THURSDAY, OCTOBER 1, 1891.

THE BACTERIOLOGICAL EXAMINATION OF WATER.

Manuel Pratique d'Analyse Bactériologique des Eaux. Par le Dr. Miquel. (Paris: Gauthier-Villars et Fils, 1891.)

THERE is probably no body of scientific men amongst whom national feeling and prejudice are so little under control as the workers in the domain of bacteriology. In perusing memoirs, text-books, dictionary-articles, and literature of every kind bearing upon this infant science, the reader must almost invariably take into consideration the language in which they are written, more especially whether German or French; and if the author belongs to neither of these rival nationalities, it is not unfrequently desirable to ascertain in which of the two camps he has been educated, for, unless this be made allowance for, a warped and often erroneous impression will be carried away.

The present work certainly forms no exception to this state of things; indeed, this phenomenon of party-spirit is regrettably prominent. Thus, in reading one of the first paragraphs, beginning with "Les premières statistiques relatives à la richesse bactérienne des eaux furent publiées par moi," and, indeed, throughout these pages we are reminded of the words of the deeply lamented *savant* who commenced his monumental work with "La chimie est une science française," and perhaps even more of the famous utterance, "L'état, c'est moi!"

Dr. Miquel's treatise, consisting of 194 pages, is divided into five chapters, dealing respectively with (1) the collection of samples, (2) the transport of the collected water, (3) the quantitative analysis, (4) the qualitative analysis, (5) the interpretation of the results obtained. On these subjects Dr. Miquel should be well qualified to write, because, as he informs us, it is only in his laboratory at Montsouris that the bacteriological examination of water has been carried on over a period of eleven years. Indeed, we know of no bacteriologist who has so entirely devoted his attention to the subject of micro-organisms in air and water as Dr. Miquel, whose name is so inseparably connected with "les organismes vivants de l'atmosphère." His energies have, however, apparently not been so successfully directed to the aquatic as to the aërial microbes, for we do not connect Dr. Miquel's name with any of the more important advances that have been made in our knowledge of the bacteria in water during the past ten years. The comparative sterility of Dr. Miquel's researches in this direction is perhaps partially to be accounted for through the extraordinarily cumbrous method of water-examination which he formerly exclusively employed, and which has placed him at a great disadvantage by the side of those investigators who at once availed themselves of Koch's methods, which Dr. Miquel, like many other French bacteriologists, has only adopted with reluctance, or almost under compulsion. The chief interest attaching to the bacteriological examination of water lies in its application to the hygiene of water-supply, inasmuch as it is all but certain that two at least of the most fatal zymotic diseases—cholera and typhoid—can be, and are, constantly propagated through the presence of specific micro-organisms in water, and indeed the majority of bacteriologists are agreed as to the particular forms responsible for these diseases. On this account it is conceived by many that the primary object of the bacteriological examination should be the search for such pathogenic microbes. This view is apparently endorsed by Dr. Miquel when he says, "Le but que doit poursuivre le micrographe dans les analyses bactériologiques de l'eau est sans contredit la découverte des organismes pathogènes"; although the logical conclusion to be drawn from the pages which follow, and in which he details the methods to be pursued in this quest, is that such an investigation is generally fraught with insuperable difficulties, and, for sanitary purposes, practically worthless. Thus, without wishing to detract from the importance of the discovery by Chantemesse, Widal, and others of the typhoid bacillus in certain waters which had been suspected of propagating this disease amongst their consumers, it is surely obvious that, even if this organism could be detected with unerring certainty in any water in which it was present, a search for this bacillus in the ordinary course of water examination would still have only a very subsidiary interest. Waters are surely not only to be condemned for drinking-purposes when they contain the germs of zymotic disease at the time of analysis, but in all cases when they are subject to contaminations which may at any time contain such germs. Sewage-contaminated waters must on this account be invariably proscribed, quite irrespectively of whether the sewage is, at the time that the water is submitted to examination, derived from healthy or from diseased persons. In the present state of our knowledge there can be no doubt that chemical analysis affords us in general a better, although a far from perfect, indication of sewage contamination than do the results of bacteriological examination. The real value of these bacteriological investigations, if judiciously applied, consists in their power of furnishing us with information as to the probable fate of dangerous organisms, should they gain access to drinking-water. It is by their means that we have learnt that many such organisms can preserve their vitality, nay, in some cases can actually undergo multiplication, in ordinary drinking-water; that they are destroyed by maintaining the water at the boiling-point for a short time; and that they are more or less perfectly removed by some processes of filtration and precipitation, whilst other processes of the same nature are worthless, or even worse.

These important results are of the greater value inasmuch as they have been obtained not only by experimenting with the few pathogenic organisms with which we are at present acquainted, but by studying the effect of these several processes on the complex mixtures of micro-organisms that are to be found in natural waters. The rapidity with which this knowledge has been acquired is due to the quantitative accuracy combined with facility of manipulation which characterize the method of gelatine-plate culture. It has been repeatedly urged against this method that it is incapable of revealing many well-known forms of bacteria which either do not grow in the gelatine-peptone medium at all, or at any rate not at those temperatures at which it still remains solid, and it

is in this respect that Dr. Miquel claims superiority for his infinitely more laborious method of "ensemencements fractionnés" in bouillon. It is obvious that labour must be no consideration if any great scientific advantage is to be attained; but, on the other hand, the unnecessary complication of processes, without corresponding benefits, must invariably lead to the retardation of scientific progress. Now, it would certainly appear that the benefits obtained by Miquel's process are in no way commensurate with the additional labour which it entails. Thus, his process is also incapable of revealing all the microbes which may be present in water, and yields at best only a closer approximation to the total number than does the gelatine method. For the general purposes of the bacteriological examination of water, however, it is of very little consequence whether the method employed reveals, say, 30, 50, 70, or 90 per cent. of the total number of microbes present, all that is required being a result which will serve for comparison. Thus, supposing it is desired to ascertain the efficiency of some process of filtration, provided that the unfiltered and filtered waters respectively are submitted to the same method of examination, the comparative result will be the same whether 50 per cent. only or all the microbes present are in both cases enumerated. Thus putting this statement to the test of actual experiment, from the results of the gelatine-plate method of examination I reported to the Local Government Board in 1886 that the average reduction in the number of micro-organisms present in Thames water effected by the sand-filtration of the several London water companies amounted to—

98·6 per cent. for the Chelsea Company,
99·1 ,, ,, West Middlesex Company,
96·7 ,, ,, Southwark Company,
98·2 ,, ,, Grand Junction Company,
96·2 ,, ,, Lambeth Company,

whilst Dr. Miquel in 1890 gives as the effect of sand-filtration on the water of the River Loire a reduction of 99·3 per cent. in one case, and 99·4 per cent. in another case. A concordance more complete than this can certainly not be demanded. Similarly it can be shown that Dr. Miquel's method of water examination has not yielded any results of importance which had not already been arrived at before by other investigators using the more expeditious method of plate cultivation. It is indeed only for such differential experiments as that referred to above that the bacteriological examination of water, in the present state of our knowledge, is really of much value, for any judgment as to the purity or otherwise of a sample of water based upon the actual number of microbes found in a given volume of it, is liable to lead to the most serious errors, in consequence of the remarkable power which some bacteria possess of multiplying to an extraordinary extent in waters of the greatest organic purity; in fact, it is precisely in the purest waters that such multiplication is often most pronounced. It is the possibility of such multiplication taking place which renders it imperative that samples of water should be submitted to bacteriological examination within a few hours of their collection. In order to overcome this difficulty, which has hitherto debarred the examination of waters from distant sources, Dr. Miquel has the samples transmitted in a box surrounded with ice; to this there are manifold objections, for the low temperature thus secured by no means completely arrests the multiplication of some bacteria, whilst it causes the destruction of others. Dr. Georg Frank, of Berlin, on the other hand, seeks to overcome the difficulty by deputing to persons on the spot the task not only of collecting the samples, but also of preparing the plate-cultures; but, considering the nature of the instructions which he finds it necessary to give to the novices to whom this work may fall, the expedient does not appear very promising. The following is a verbatim extract from these instructions recently published in a German scientific journal of repute, which surely demands no comment:—

"The person commissioned with the collection of the sample takes off his coat, turns up his shirt-sleeves on both arms, fastening them so securely that they cannot fall down of themselves. Then he washes his hands and arms most carefully with soap and brush to above the elbow-joint. Special care must be bestowed upon the cleansing of the finger-nails, which must if necessary be treated with the nail-file. Finally, the person in question dries himself with a clean towel."

We take it that the value of results depending upon manipulations carried out by persons requiring these instructions would be such that it would be no loss if they were dispensed with altogether. Indeed, unless the bacteriological examination of water be invariably carried out by qualified persons, and by them employed only in cases where it is really capable of rendering service, it is certain to fall into that disrepute which has so frequently been drawn down upon the chemical examination of water through incompetent analysts. Indeed the bacteriological method has already seriously suffered in public estimation through the contradictions which have resulted from the attempts made in some quarters to classify waters according to the number of microbes revealed on cultivation. Such arbitrary standards have already done much mischief in the case of the chemical analysis of water; in the bacteriological examination they are still more reprehensible, and it is deeply to be regretted that Dr. Miquel, in this most recent work on the subject, should seek to perpetuate a system of standards which experience shows to be quite untenable.

The work concludes with some excellent recommendations as to the sterilization of water for drinking-purposes, a subject which cannot be too frequently brought into public notice, for, using Dr. Miquel's own words, "la vie d'un homme a bien sa valeur à côté du prix insignifiant auquel revient le litre d'eau purgée de germes qu'il peut consommer en vingt-quatre heures."

PERCY F. FRANKLAND.

EPIDEMIC INFLUENZA.

Epidemic Influenza: Notes on its Origin and Method of Spread. By Richard Sisley, M.D. (London: Longmans, Green, and Co., 1891.)

THE object of this brief treatise, which was prepared before the issue of the Report of the Local Government Board, is to prove the doctrine, widely held by physicians of eminence in the eighteenth century, that influenza is contagious, or, more strictly speaking, infectious, and therefore, in the opinion of the author, fit

to be included among the diseases of which notification is locally compulsory. The book is somewhat peculiar in its arrangement, but in the essential qualities of impartiality and clearness leaves nothing to be desired. Many readers who do not require more than specimens of evidence, will thank Dr. Sisley for compressing the digest of "many thousands" of notes into such narrow compass; but other minds will require a chain of which every link is massive, to guide them to the point of view whence practical conclusions are palpable. If the manner of statement is somewhat bare, and examples rather scanty, in the exposition of a strong but disputed case, the facts brought forward bear none the less value in their neutral setting, and go far to justify the proposition with which he confronts us at the outset, derived from a study of the distribution of the disease and from its pathological character. Valuable assistance from Dr. Klein, Prof. Fleming, and many others, has enabled him to include in his pages some interesting matter relating to the microbic nature of the epidemic and its relation to a similar disease in animals. After all that has been conjectured on the latter point, it appears that evidence of any unusual prevalence of influenza among animals at the time is still wanting.

The original seat of influenza, which has been obscurely indicated in previous times as lying somewhere "in the East," has now been discerned in Mongolian and Chinese territory, for we have two independent accounts, each speaking of influenza as not uncommon in some parts of China. In Mongolia "it seldom proves fatal, but travellers are careful to avoid it, and no one would think of using the pot or ladle of a family suffering from this sickness." If the disease is sporadic and endemic in these countries, the population may be to some degree protected against epidemic outbreaks, for we have seen in Europe that the tendency to spread is much less marked in a second invasion occurring within one year, and least, on the whole, in those places where it was previously most severe.

The notes from Bokhara, translated in this volume, are of great importance, for they show how a wet spring had turned the neighbouring country into a perfect marsh, from which, when the hot weather set in, poisonous exhalations were given forth, and how the people, crowded together with horses, cattle, and sheep between high walls, distressed and weak with starvation and disease, were attacked much earlier than usual, in the first heat of summer, with malaria, and how this was quickly followed by an epidemic of influenza, reaching its height in July 1889. The extension of the disease westwards from Bokhara by the flight of convalescents to Russia, and eastwards by caravans to post-stations in Siberia, has been noticed in the official Report, and completes the evidence connecting the European epidemic with the miserable condition of an Asiatic town. Upon such a soil, influenza sprang into fatal activity, and acquired, as we may fairly infer, a particular virulence. In similar conditions, amid the filth, floods, and famines of Asiatic countries, cholera and other plagues of men and animals have been evolved and have set forth on their destructive march.

By reports from several medical officers, and by a number of charts showing the curve of prevalence of the disease in English and foreign cities, Dr. Sisley shows that we have no experience of any sudden prostration of a large population within a few days, such as was formerly supposed to occur; but that the rise is always gradual from a few cases to hundreds and thousands, the maximum usually occurring from one to two months after the first cases in the locality have been noted. Last century Dr. Haygarth had been fortunate in discovering the person who brought the infection to each place in his district. If equal pains had been taken in 1890, when the disease was on its way to us from Russia, the persons who conveyed it from country to country might, no doubt, have been identified. The author has not been able to find a single instance in which there was a sudden infection of a large number of people without the previous existence of cases of the disease; and wherever its course was studied with care, it was seen to spread in the same way as other infectious diseases. But the "atmospheric" doctrine, though previously disproved with regard to rabies, cholera, and pestilence in general, still finds a stronghold in consumption and influenza.

The classic examples of ships supposed to have been attacked on the ocean by wind-borne influenza, as well as those of towns supposed to have been prostrated "in a single day," really bear testimony to the insidious growth of the disease and to the necessity of early recognition. Neither in this volume nor in others on the same subject is the fact sufficiently dwelt upon, that the geographical distribution of this and of previous epidemics in successive weeks and months was wholly unlike what would have occurred if the germs had been largely spread, either by lower or by upper atmospheric currents.

The total exemption of lighthouse-keepers, deep-sea fishermen, and unvisited islands, is scarcely noticed by Dr. Sisley, but he considers the rarity of influenza among prisoners to have been due to their removal from sources of contagion, and relates a very interesting case of apparent infection of a man on his way home from a light-ship through contact with the crew of a fishing-boat, said to be in good health.

Dr. Sisley concludes that there is no convincing proof of transmission through unaffected persons, letters, &c.; but a series of cases each of considerable weight surely amounts to evidence strong enough to justify some precautions, such as would be taken with the organic dust from more serious diseases, *e.g.* scarlet fever and diphtheria, which are so transmissible. There is happily a great deal in common in the mode of spread of most zymotic diseases, and disinfection as usually practised could hardly be misapplied to influenza. The same may be said with regard to isolation, for no attack, however trivial in itself, is a matter of indifference to the public, if it may result in widespread illness, loss of work, and distress. A short retirement is desirable in the interest both of the patient and of the public. But Dr. Sisley can hardly desire that notification should take place on exactly the same lines as that of other diseases, for local authorities would with reason wince at the expense; and unless the notification were a national undertaking, no district would be adequately protected thereby from imported cases. Complete and national measures of notification and isolation, with the co-operation of local authorities, would be much more

likely to be effectual. An expenditure of one-fiftieth of the cost of the recent epidemic would probably secure the country from any such infliction in future. But we must admit that without a somewhat strict supervision at ports of entry during the period of prevalence in other countries, and without provision for the segregation of slight or suspected cases during that period, mere notification would not be likely to put a stop to the spread of influenza. The early cases are worth taking a great deal of trouble to discover and isolate. When once many cases have occurred in a locality, the further progress of so protean a disease is difficult to arrest. The best chance of averting an epidemic must be sought in scrupulous care for early isolation, in tracing the movements of travellers from infected towns, and in the increased practice of ventilation in private houses and in public gatherings. Like typhus, influenza seems incapable of inflicting much damage except through the medium of close, confined, and impure air, and where measures of isolation and disinfection are used it seldom spreads. But the infectious character of influenza must be internationally recognized before protective regulations can achieve a full measure of success.

R. Russell.

GENERAL CHEMICAL MINERALOGY.

Allgemeine Chemische Mineralogie. Von Dr. C. Doelter, O. Professor der Mineralogie an der K. K. Universität Graz. With 14 Figures in the Text. (Leipzig: W. Engelmann, 1890.)

MINERALOGY, at first purely descriptive, has been raised to the dignity of an experimental science by the application of the principles of chemistry and physics. The writer of a mineralogical text-book is thus met at the outset with the difficulty of deciding what amount of knowledge of chemistry and physics to assume in his reader. With regard to the chemical side at least, the rule appears to be to assume that he knows very little, and yet, somewhat inconsistently, to make the exposition of the atomic theory and the fundamental principles of chemistry so brief as to be of little service to one who has had no previous acquaintance with the subject.

The author of the present, in many respects useful and suggestive, book follows the same lines. The whole account of the fundamental chemical theories occupies about ten pages of the introduction. The same fault will be found in other parts of the book: *e.g.* it would be difficult to say to what class of reader a large portion of the chapter on chemical analysis would be useful. In his endeavour to introduce as many extracts as possible from the current literature of the subject, the author allows himself in many places to become somewhat sketchy. In spite of this, the book, with its wealth of information upon points which have not hitherto found a place in ordinary mineralogical text-books, will be found to give a very good idea of the present state of mineralogical science from a chemical point of view.

The arrangement of the book is in seven sections, viz. (1) introduction; (2) chemical crystallography; (3) chemical analysis of minerals; (4) synthesis of minerals; (5) metamorphism of minerals; (6) formation of minerals in nature; (7) chemical composition and constitution of minerals.

In the introduction, containing an account of the atomic theory and its consequences, one or two suggestive ideas will be found: *e.g.* the correspondence, pointed out by Tschermak, between the chemical law of multiple proportions and the crystallographic law of simple parameter ratios; and also the analogy between the law of constant proportion by weight and the fundamental crystallographic law of constancy of angle. The subject of chemical crystallography receives very full treatment. Here the reader is initiated into the mysteries of chemical and physical isomerism, polymorphism, enantiotropy, isomorphism, isodimorphism, isogonism, morphotropy, &c.; and if the perusal of this section, as well as of the last, on the constitution of minerals, shall leave him with a rather confused and unfavourable idea of the subject, the fault should perhaps be rather attributed to the present imperfect state of our knowledge than to the author. At present it is in most cases impossible to say whether bodies are polymeric, metameric, or chemical isomers.

As regards isomorphism, if the formation of mixed crystals is to remain the test, the original definition of Mitscherlich must be modified to suit the fact of the formation of mixed crystals from compounds of not precisely analogous chemical composition. Thus, according to modern views, isomorphism is in some degree to be deposed from its proud position as an infallible guide to chemical composition. The insidious nature of the attack upon this ancient stronghold of the faith may be judged by a comparison of one of the latest definitions of isomorphism with the original definition of Mitscherlich. According to the latter, isomorphism is the power which two or more compounds of *analogous* chemical composition possess of crystallizing in the same or similar crystalline forms, and of mixing in varying proportions to form homogeneous crystals. The latest definition is that bodies are isomorphous which, with *for the most part similar* chemical composition, possess the property of crystallizing in similar crystalline forms, and of forming mixed crystals which morphologically and physically graduate into each other. Such a change it is expected would lead to a considerable simplification in many of the formulæ which have been made unnecessarily complicated in order to comply with the requirements of Mitscherlich's definition.

The section on chemical analysis of minerals is one of the least satisfactory in the book. Short summaries of analytical methods can be of little service to any class of reader. Amongst matter which will not be generally found in the ordinary chemical text-book, this section contains some account of microchemical reactions, of the methods for the mechanical separation of minerals, so as to insure pure material for analysis, and directions for the course of analysis to be pursued in the case of the more important minerals.

The important subject of mineral synthesis receives more complete treatment than any other in the book. The section contains general accounts of the various methods for the artificial production of minerals by chemical reactions, fusion, sublimation, electrolysis, diffusion, &c., with detailed descriptions of the apparatus required.

The sections on the metamorphism of minerals, and on the formation of minerals in nature, will be found of great interest to the petrologist. Here are described the effects on minerals of heat, of gases at high temperatures, of fusion, of fused magmas, of water containing carbonic acid, &c. In the last section, dealing with the composition and constitution of minerals, the present imperfect state of our knowledge is brought prominently to light. The battle is still being fought between the so-called chemical, liquid, and crystal molecule; between constitutional and empirical formulæ. Mineralogists are beginning to understand that it is impracticable to attempt to use for complicated minerals principles which are only applicable to volatile organic compounds, and the idea is gaining ground that many minerals are molecular compounds only capable of existing in the solid state, the crystal molecule being built up of different chemical molecules.

The author intends to supplement the present work by another, entitled "Chemical Mineralogy," in which the composition, synthesis, &c., of each individual mineral will be treated more particularly. The present volume is intended as quite a general treatise on the subject of mineral chemistry; in fact, we cannot help thinking that in many parts the treatment is far too general, and that the book has been partially sacrificed for the sake of the volume that is to follow. The value of the book is increased by the lists of references to the literature which precede each section. G. T. P.

OUR BOOK SHELF.

Bush Friends in Tasmania: Native Flowers, Fruits, and Insects, drawn from Nature, with Prose Descriptions and Illustrations in Verse. By Louisa A. Meredith. Executed by Vincent Brooks, Day, and Son. (London and New York: Macmillan and Co., 1891.)

UPWARDS of thirty years ago Mrs. Meredith gave the world a volume containing admirable coloured figures of a selection from the many beautiful plants and insects that inhabit her island home, Tasmania; and now, in the evening of a long life, she has travelled to the old country to publish a second volume, which is to be the last. Her purpose achieved, she "hopes to return and end her days among her children in that pleasant colony," which has given a brighter home to so many of our kith and kin. Lovers of the beauties of Nature in this country will find much pleasure and instruction in this second volume from that talented lady's pen and pencil, and will be able thereby to form some conception of the totally different kind of vegetation from our own that clothes this remote southern island, as well as the great Australian country, for it is only a part of the same flora. To the colonists themselves the book will be even more attractive, as a means of becoming acquainted with the names and affinities of the beautiful objects with which they are surrounded. It will also, it is to be hoped, teach them to prize and preserve these rare and precious gifts. Like all true lovers of Nature, Mrs. Meredith deplores the wanton destruction of rare flowers near Hobart by thoughtless or greedy persons whose only aim seems to be quantity.

The botanical part of Mrs. Meredith's book is perfectly trustworthy, having been scrutinized by so eminent an authority as Sir Joseph Hooker; and Prof. Westwood furnished the names of the insects.

Some of the poems have a special interest in connection with the early history of the settlement of Tasmania. Notably an "Old Story" of 1834, which narrates the massacre by aborigines of a whole family—father, mother, and seven children.

The Elementary Geometry of Conics, with a Chapter on the Line Infinity. By C. Taylor, D.D. (Cambridge: Deighton, Bell, and Co., 1891.)

DR. TAYLOR'S "Geometry of Conics" is so well known, and has met with such acceptance—this is the seventh edition, revised—that we are not called upon to give a detailed account of it. Two additions, however, claim a brief notice. A new chapter (xii.) contains "a course for beginners," in which students who prefer to take the three conics separately have a selection of articles, from the text, indicated for a first reading. Further, a set of duplicate proofs is given in outline, the completion of which is left to the reader. The other novelty (chapter xi.) is "a new treatment of the hyperbola." This is the expansion of a paper which the author read before the Association for the Improvement of Geometrical Teaching, in January 1890, and of which the President (Prof. Minchin) is reported to have said: "One thing that struck him about the paper was, that Dr. Taylor arrived at points on the curve in a very much more rapid and simple way than any he had previously known of." The author remarks that it is in accordance with the historical order to draw the asymptotes before tracing the curve, for the hyperbola seems to have been discovered from its "equation" (A.I.G.T. Report, 1890, p. 12).

It is somewhat remarkable that Dr. Taylor does not give a proof of this equation. We append one. Taking his figure on p. 103, we draw the second asymptote. Now draw PM parallel to Cp, cutting the axis in K, and the second asymptote in M: then,

$$\begin{aligned}4CM \cdot MP = 4MK \cdot MP &= (MP + MK)^2 - (MP - MK)^2\\ &= Cp^2 - KP^2 = \lambda^2(pN^2 - PN^2) \text{ (where } \lambda \text{ is a constant)}\\ &= \lambda^2(Sp^2 - SP^2)\\ &= \lambda^2(Sp^2 - pY^2) = \lambda^2 \cdot SY^2 = Ca^2 = a^2 + b^2.\end{aligned}$$

Again, let PQ be any chord meeting the asymptotes in p, q; and let Ql, Pm, parallel to Cp, Cq respectively, meet those lines in l, m. Then we have

$$\frac{Pq}{Cm} = \frac{Pp}{pm} = \frac{pq}{Cp} = \frac{Qq}{Ql},$$

$$\therefore \quad \frac{Pq}{Qq} = \frac{Cm}{Ql} = \frac{Cl}{Pm} = \frac{pQ}{Pp};$$

hence

$$Pp = Qq, \text{ and } Pq = pQ.$$

Other properties occur to us, but the above are classic properties of the curve, and the wonder is that Dr. Taylor has not applied his new treatment to obtain them. There is no suggestion that they can be so obtained, either in the book or the original paper as printed in the A.I.G.T. Report. R. T.

Les Engrais Chimiques. Par Georges Ville. Septième Édition. (Paris: M. Engel, 1890.)

THIS is a new edition of the author's lectures on chemical manures, which were first published in 1868, and which have been translated into seven languages. An English edition, by Mr. Crookes, was published in 1879. The sixth French edition has been out of print for about ten years, and during that time the price of chemical manures has considerably declined, on an average about 40 per cent. On this account the author has introduced, at the end of the volume, a chapter containing new formulæ for mixed manures, based on considerations of market value and more complete knowledge of the requirements of

crops. Thus, potassium chloride replaces potassium nitrate in the manure for leguminous plants, and in some cases a mixture of potassium chloride and ammonium sulphate replaces potassium nitrate; and a few other alterations are suggested in the treatment of various crops. Thomas's basic cinder is not mentioned as a source of phosphoric acid. The lectures themselves, and some controversial matter, are reprinted in their original form, and but little new matter is added.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Bird-Collections in the Oxford University Museum.

DURING a recent visit to Oxford I took the opportunity of examining the collection of birds in the University Museum, and beg leave to offer a few remarks upon its condition.

First, as regards the mounted specimens, there are three series belonging to this category:—

(1) The general series in the Central Court. This numbers about 1100 specimens, which are contained in twelve cases, placed in opposite rows of six each, but rather mixed up with mammals, shells, and other objects. The specimens are arranged according to Gray's "Genera," and in most cases correctly named. But many of them are in bad order and not well set up, and should be replaced by fresh examples. The whole series requires renovation and rearrangement, according to some modern system, and the orders and families should be designated by labels, and distinctly separated one from another.

(2) The collection of Arctic birds formed by Mr. J. Barrow, F.R.S., and presented to the Museum by that gentleman. This interesting collection, which has been well described by Mr. Harting in the *Ibis*, is placed in the gallery. It is well mounted and correctly named. But it is a question whether it is desirable to keep it apart from the general series.

(3) The British series, also placed in the gallery, which is in fair order, although it also requires revision and rearrangement according to some modern system. It ought not to be difficult to find some member of the British Ornithologists' Union to undertake this task, provided that the authorities will allow him a "free hand."

Besides the mounted specimens, there are, as I understand, about 4000 skins of birds, most of which are "put away" in boxes in various parts of the building. Of these, the only portion that I was able to see was the Bornean collection formed by Mr. Everett, and partly described by Dr. Bowdler Sharpe in the Zoological Society's Proceedings. These are placed in some drawers in the main hall. The other skins are stated to be "boxed up," and are kept partly in a room on the ground floor, and partly in some "upper chamber," to which no ready access is possible.

I venture to suggest that one of the side rooms in the Museum should be cleared of its contents, and devoted entirely to the bird-skins, and that they should be arranged there in cabinets, so as to be accessible to the ornithologist. It is hardly right for a great and rich University to accept collections from persons who, in the words of the late Prince Bonaparte put forward on a similar occasion, "croyant qu'ils travaillaient pour la science, non pas travaillés que pour les mites." I may add that any assistance that I can give in carrying out this reform will be most gladly rendered. P. L. SCLATER.

3 Hanover Square, London, W., September 4.

Variation and Natural Selection.

IN Prof. C. Lloyd Morgan's Presidential address to the Bristol Naturalists' Society, on "The Nature and Origin of Variations" (of which he has kindly sent me a reprint from the Society's Proceedings), there are one or two points on which there seems to me to be a slight misconception; and as the difficulties suggested have probably occurred to other naturalists, I wish to make a few observations in the hope of throwing a little light on this obscure subject.

After referring to the proofs of the variability of species in a state of nature which I have adduced in my "Darwinism" (to which proofs Prof. Lloyd Morgan has made some important additions in his recent work on "Animal Life and Intelligence") he remarks:—"We have been apt to suppose that a species is so nicely adjusted to its surrounding conditions that all variations from the type, unless of a very insignificant character, would be rapidly and inevitably weeded out. This, it is clear, is not true at any rate for some species." And a little further on, after discussing the question whether variations in all directions occur in equal proportions—an equality which does not appear to me to be at all necessary, or to have been ever suggested as occurring—he says: "And the candid biologist must, I think, admit that the evidence in Mr. Wallace's third chapter, while conclusive as to the occurrence of variations, gives on analysis little or no evidence of any selective agency at work."

The difficulties here stated appear to me to depend, chiefly, on not taking account of some important facts in nature. The first fact is, that the struggle for existence is intermittent in character, and only reaches a maximum at considerable intervals, which may be measured by tens of years or by centuries. The average number of the individuals of any species which reach maturity may be able to survive for some years in ordinary seasons or under ordinary attacks of enemies, but when exceptional periods of cold or drought or wet occur, with a corresponding scarcity of certain kinds of food, or greater persecution from certain enemies, then a rigid selection comes into play, and all those individuals which vary too far from the mean standard of efficiency are destroyed.

Another important consideration is that these epochs of severe struggle will not be all of a like nature, and thus only one particular kind of unbalanced or injurious variation may be eliminated by each of them. Hence it may be that for considerable periods almost all the individuals that reach maturity may be able to survive, even though they exhibit large variations in many directions from the central type of the species. During such quiescent periods, the chief elimination will be among the young and immature. Thus, with birds probably nine-tenths of the destruction occurs among the eggs and half-fledged young, or among those which have just escaped from parental care; while those which have survived to breeding age only suffer a slight destruction in ordinary years, and this may occur partly among the less experienced, partly among those which are old and somewhat feeble.

The severe elimination that occurs in the earlier stages may be thought to be accidental, but I doubt if it is really so except in a very small degree. The protection and concealment of the eggs and young in the nest will depend chiefly on the mental qualities or instincts of the parents, and these will have been always subject to a rigid selection owing to the fact that those with deficient instincts will leave fewer offspring to inherit their deficiency. And with young birds of the first year there will be an equally rigid selection of the incautious, and of those who are deficient in any of the sense-perceptions, or are less strong and active than their fellows.

The proof that there *is* a selective agency at work is, I think, to be found in the general stability of species during the period of human observation, notwithstanding the large amount of variability that has been proved to exist. If there were no selection constantly going on, why should it happen that the *kind* of variations that occur so frequently under domestication never maintain themselves in a state of nature? Examples of this class are white blackbirds or pigeons, black sheep, and unsymmetrically marked animals generally. These occur not unfrequently, as well as such sports as six-toed or stump-tailed cats, and they all persist and even increase under domestication, but never in a state of nature; and there seems no reason for this but that in the latter case they are quickly eliminated through the struggle for existence—that is, by natural selection.

One more point I will advert to is Prof. Lloyd Morgan's doubt, in opposition to Mr. Ball, "whether a thicker or thinner sole to the foot is a character of elimination value, whether it would determine survival or elimination, and make all the difference between passing or being plucked in life's great competitive examination." This seems to me to be a rather unfortunate objection, since, in constantly recurring circumstances during the life of a savage, this very character must be of vital importance. Whether on the war-path, or in pursuit of game,

or when escaping from a human enemy or from a dangerous animal, the thickness of the sole, its insensibility to pain, and its resistance to wear and tear must have often determined life or death. A man who became sore-footed after a long day's tramp, or one whose thin sole was easily cut or torn by stones or stumps, could never compete with his thicker soled companions, other things being equal; and it seems to me that it would be difficult to choose a single physical character whose variations would be more clearly subject to the law of selection.

With the greater portion of Prof. Lloyd Morgan's very interesting address I am in perfect accord, and it is because his remarks and suggestions are usually so acute and so well founded that I have thought it advisable to point out where I think that his objections have a less stable foundation.

ALFRED R. WALLACE.

A Rare Phenomenon.

THE rare phenomenon to which your two correspondents refer in their letters in your last issue (p. 494) was visible here at precisely the same time, and, viewed from Nottingham Forest, it presented a most interesting sight. It is curious that, as both the time and duration of the phenomenon coincide with its appearance here, its characteristics should be so dissimilar. It had more the appearance of a well-defined display of the aurora. Rays of light springing from the horizon penetrated high into the heavens, lasting about 10 or 15 seconds, and then disappeared, others taking their places. Its centre appeared to me to be almost due north, and, from notes made at the time, the beams or luminous rays reached an angle of about 50°, stars being visible through them. There was no arc visible of the character described by your correspondents, but vertical changing rays, several of which were distinctly orange-tinted.

Nottingham, September 26. ARTHUR MARSHALL.

YOUR columns record, from Ireland and Scotland, observations of the aurora to which I called attention last week. It was seen also in Warwickshire, the coruscations being so marked as to remind my informant of the search-light at the Naval Exhibition. Mr. E. B. Knobel informs me that, from 8 to 10 p.m. on the 11th, during which time the appearance was visible, active magnetic disturbances were noticed at the Royal Observatory, Greenwich, illustrating the close connection which has been established between auroral and magnetic phenomena.

W. TUCKWELL.

IT may be of interest to your readers to know that the "rare phenomenon" mentioned (p. 494) was seen by me from Ryde, I.W., on Friday, the 11th. A streak of light (at first thought to be a ray proceeding from a search-light), was visible near the Pleiades, at about 9.30, extending over an arc of about 45°, the width being probably about 1°. It gradually faded away, and at 10 no trace of it was left. F. C. LEVANDER.

30 North Villas, Camden Square, N.W., September 28.

Instruments in Just Intonation.

AS you have raised once more the question of justly intoned instruments, may I offer the following remarks? It does not seem likely that any arrangement for the organ would be practically adopted unless it permits as much freedom of modulation and of execution as that of equal temperament. To permit perfectly free modulation, with practically perfect intervals, nothing short of the cycle of fifty-three will suffice. Now to construct a key-board with fifty-three notes to the octave which can be played upon with the facility of a twelve-note key-board seems impossible. But the problem may be approached differently: as it is only necessary to use twelve notes at a time, the key-board might remain as it is, and only a mechanical device would be required to make these twelve keys correspond to the right twelve out of fifty-three pipes; if the services of an assistant be allowed (as is often necessary on large organs) the mechanical difficulties could easily be overcome. For example, arrange a number of studs—say 117, as suggested by Dr. Ellis—as a "duodenarium," and connected electrically to the fifty-three trackers; *i.e.* each tracker would be connected to two or three studs—$B^3\flat\flat$, $C^4\flat\flat\flat$, $A_3\sharp\sharp$ studs to tracker 46 for instance. Opposite these studs would be another set of 117 connected to the twelve keys, *e.g.* C, $B_2\sharp\sharp$, $B_3\sharp\sharp$, $D^2\flat\flat$, &c., all to the key C. Between the two sets of studs would be a frame carrying twelve contact pieces; the frame would then be moved along guides by the assistant, so that the twelve keys were electrically connected to the right duodene of studs, and hence could be made to open the right group of pipes.

Thus the only alteration in printing required would be to mark the duodene on the music. All the extra complication would be thrown on the mechanical arrangements, and the organist would be left in the same position as now. It seems to me that any more complicated key-board would fail in a large organ, through overburdening the organist.

ROBT. A. LEHFELDT.

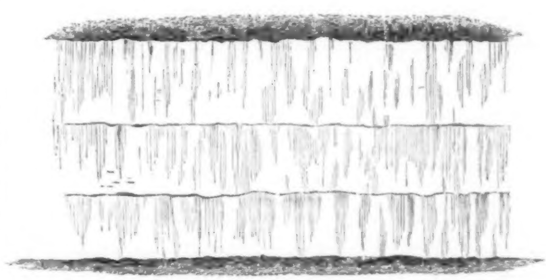
Firth College, Sheffield, September 14.

Unusual Frost Phenomenon.

THE following is extracted from a letter dated Dubbo Creek, near Tumut, New South Wales, July 26, 1891:—

"I noticed the other day a strange effect caused by the late very hard frosts. It was a peculiar upheaval of the crust of the ground by a mass of innumerable threads of ice taking the form of spun glass or fine asbestos fibre. There were five layers of this ice-fibre, the uppermost bearing the raised earth-crust. Every night's frost was shown by its distinctive layer of fibres.

"As perhaps you may never have seen this form of ground frost, I append a rough sketch of its very singular appearance.



I have only shown three layers; there were five, but this may give you some idea of its appearance—quite a columnar basaltic appearance.

"Every morning here after a sharp frost, the whole of the ground, where not covered by grass or rubbish, is raised up thus. On the sides of the cuttings and banks of our claim, these ice-fibres may be seen projecting from the walls in bunches of snowy filaments, like spun glass. The sun, however, soon causes them to drop off, and they lie in heaps of some six inches in depth." A. H. WHITE.

Richmond, Surrey.

The Destruction of Mosquitoes.

ON two occasions, when proceeding northwards to Arctic Norway, I was much interested in observing the fact that the plague of mosquitoes, which is so intolerable there, especially prevails in latitudes beyond the northern range of the swallow.

This may possibly be a mere coincidence, but I think it is not —an opinion strongly supported by another and very broad fact, viz. that in a given district in our own country the gnats become more abundant immediately after the departure of the swallows, martins, &c. If this view is correct, the protection of these birds should be added to the devices named in your review of "Dragon-flies *v.* Mosquitoes." Such protection is very different from the indiscriminate sentimentalism about "small birds" which breaks out periodically at this season in the newspapers, and includes such feathered vermin as the thick-billed, seed-grubbing, pea-shelling, graminivorous sparrow among the objects of its tenderness. W. MATTIEU WILLIAMS.

The Grange, Neasden, N.W.

A Tortoise inclosed in Ice.

DURING the last winter there was a good deal of correspondence in the columns of NATURE regarding the revivability of fish and insects that had been frozen hard. A similar phenomenon with regard to the tortoise having recently come under my notice, it may perhaps be interesting to some of your readers to have it put on record.

Some friends of mine have one of the small water-tortoises that are occasionally exposed for sale in the City. Last winter, this tortoise was inadvertently left in his small pond, the water of which froze completely into one block of ice, inclosing the tortoise. When the thaw came, the creature was found alive and flourishing. I especially endeavoured to ascertain whether the tortoise had been absolutely and completely *inclosed* in his icy casing, or whether he had been simply frozen into the ice, but partly inclosed and partly free. Unfortunately, however, in spite of cross-examining several of the family, I was unable to obtain a perfectly clear and definite statement on this point: one of my friends, however, declared that, if not completely encased, at any rate only the arch of the tortoise's back was free. This is, however, sufficiently indefinite to debar one from asserting that all access of air was denied to the tortoise; and that is the point on which my interest chiefly centred.

F. H. PERRY COSTE.

7 Fowkes Buildings, Great Tower Street, E.C., September 25.

The Soaring of Birds.

I HAVE read with much interest Mr. Peal's account of the soaring of vultures, pelicans, adjutants, &c., over the plain of Upper Assam (NATURE, May 21, p. 56). Their manner of flight is identical with that of seagulls and harriers over the Canterbury Plains in New Zealand, which is about 150 miles long and 45 wide in its widest part. These birds begin to soar at a height of about 200 feet, and rise in slanting spirals to 2000 feet and under. The gulls are much the most numerous, and flocks of them may be seen soaring nearly every fine day in summer. Sometimes a number assemble, and after going round in circles for a short time, without rising, or rising very little, they come down, the condition of the air being apparently unfavourable for soaring. Whenever I have seen a flock finish an ascent, they all reached the same height, which is consistent with the supposition that they go as high as they can. They never remained at the limit of their ascent even for a short time, but separated, sailing away downward to great distances.

The explanation of soaring given by Mr. Peal can hardly be the true one. Bishop Courtenay has shown its inadequacy by proving that a bird in a uniform horizontal current is in no respect more able to support himself than in a calm. Though carefully looking for it, I have never been able to see the descent which Mr. Peal supposes to be made (he does not say that he has seen it) when the bird is going with the wind.

The soaring of birds shows plainly that the velocity of the wind over a flat country does not increase with the height in a perceptible degree up to great heights. If there were such an increase at anything like the rate near the ground, a bird soaring would be out of sight long before he could reach 1000 feet, but birds seem to drift horizontally at nearly the same speed during the whole of their ascent. The increase of the velocity of the wind with the height may be studied by observing the behaviour of smoke or steam carried along: near the ground the increase is easily seen, over 20 feet it is very small, over 50 seldom perceptible, a wreath of smoke over that height being carried along without any relative motion of the parts, or so little that it could be of no use in soaring.

In a description of the sailing flight of the albatross (NATURE, vol. xl., p. 9) I mentioned that when the wind is at right angles to the course of a steamer attended by a flock of albatrosses, some of them occasionally follow the vessel not far astern in undulating lines, rising against the wind and falling with it, and turning alternately right and left; also that seagulls do an imperfect imitation of this kind of flight over flat country, nearly touching the ground at each descent, as the albatrosses nearly touch the sea. The gulls are evidently unable to reach the height from which the previous descent was made without flapping their wings a few times during the second half of each ascent. Without doing this, they would soon come to the ground, though using the differential motion of the air, where it is at its maximum, to the greatest advantage possible. It seems, therefore, that soaring at great heights cannot be explained on the same principle as the sailing flight of the albatross, whose movements are confined to a comparatively thin stratum of air next the sea, in which the velocity of the wind increases rapidly with the height.

In Lyttelton Harbour, N.Z., which is surrounded by hills except at the entrance, the gulls soar by using the upward current on the slopes, rising in spirals in precisely the same manner as when soaring hundreds of feet above the plain. The motive power in the former kind of flight is evident, and perhaps throws light on that of the latter. Standing on a slope of about 20°, and about 100 feet above the sea, I saw a flock of gulls sitting on the water. A breeze sprang up, and the whole flock began to ascend over the slope. Being constantly among the shipping they are very tame, and several came within 12 feet of me. When moving against the wind their motion with respect to the earth was very slow, so that I had a good opportunity of seeing if there was any vibratory movement of the wings, but no movement of any kind was visible. The ascent of birds over a slope by means of the current flowing up it, and their descent in long inclines at a small angle with the horizontal, show that rapid motion through the air causes a great resistance in opposition to gravitation, which resistance has not yet, I believe, been accounted for quantitatively on mechanical principles.

The explanation of soaring at great heights which presents the fewest difficulties seems to me to be—that it is done by means of upward currents. This has been suggested by several observers, its main difficulty being the uncertainty that there are such currents of sufficient strength. I shall try to show that upward currents may be caused in two ways, but it would not be possible to give a direct proof that the currents so arising are strong enough. If, however, birds are seen to soar when one or other of these causes is present, there is a strong probability that they are true causes of soaring.

Everyone who has watched the working of a windmill must have seen that the force of the wind varies frequently, and sometimes rather suddenly. It is evident that there must be an ascent of air in front of a current moving faster than the average speed, and a descent of air behind it. As an example of this, a cold south-west wind was blowing, with showers of rain at intervals, accompanied, as often happens, by increased force of the wind. I saw a flock of gulls soaring in front of one of these squalls. There can, I think, be little doubt that there was an ascending current, of which the gulls took advantage.

Mr. W. Ferrel has shown ("Popular Treatise on the Winds") that if the rate of fall of temperature with increase of height be greater than the rate of dynamical cooling of an ascending current, the atmosphere is in an unstable state—that is, if by any cause a mass of air be started in an upward direction in such an atmosphere, the density of the ascending air is less than that of the surrounding still air, so that the former would be driven upwards, and an ascending current established, which would tend to rush up to the top of the atmosphere if the instability, consequent on the vertical decrease of temperature, should extend all the way up; but if the instability did not extend to the top, then, at its limit, the impelling force would cease, and friction would soon bring the ascending current to rest. Conversely, in an unstable atmosphere, if a mass of air be started downward, the density of the descending air is greater than that of the surrounding still air, and the descent tends to continue down to the ground. Mr. Ferrel says (p. 440):—"The unstable state in unsaturated air occurs mostly on very dry and sandy soils with little heat conductivity, when the weather is very warm, and the heat rays of the sun are unobstructed by any clouds above. The heat thus accumulates in the surface strata of the soil and the lower strata of the atmosphere, and thus is brought about the unstable state, at least up to a low altitude, even in clear dry weather." And in speaking of what may be called a multiple tornado (p. 412): "As the tornado originates in air in the unstable state, it often happens that there is about an equal tendency in the air of the lower stratum to burst up through those above at several places in the same vicinity at the same time."

This tendency of the lower strata to burst up in separate spots may exist where the instability is much less than that required to cause a tornado, as in the case of a plain strongly heated by the sun, and in the absence of any gyratory motion round the centre of an ascending current, there would be no whirlwind, only a

quiet ascent of air, in a slanting direction if there were any wind. Such ascending currents may be of small area, not much larger than the circles described by birds when soaring. It seems possible that the object of describing circles may be to keep within the ascending current, though it is true they sometimes describe circles when the ascending current is up a slope and not limited to a small area. If a plain much heated by the sun border on the sea, ascending currents will soon start a sea-breeze, and the cold air from the sea will soon restore the stability of the atmosphere. In summer the sea-breeze blows over the Canterbury Plains four or five days a week, beginning between 8 a.m. and noon. When delayed till near noon, the soil and lower strata of air are much heated, and as the previous nights are cool, the conditions for causing the unstable state are present. I long ago remarked that the best time to look out for soaring birds is at the commencement of the sea-breeze when it is late. Soaring is much oftener seen here in summer than in winter, and is, I believe, more common, and the species of soaring birds more numerous, and the birds larger, in hot than in cold climates—that is, in climates where the unstable state of the atmosphere is oftenest caused by the sun's heat.

Mr. Peal says: "That there are no uprushes of air I have fairly good proof in the small tufts of cotton from the *Bombyx malabaricum* which cross the field of my telescope when examining the Noga Hills at ten, twenty, or thirty miles; these are always beautifully horizontal at elevations of from 200 to 2000 feet, coming from the plains and hills to the north-east of us." The presence of light bodies at great heights seems to show that there *are* upward currents: no doubt uprushes of air at a large angle with the horizontal, and of considerable area, might be detected by a careful observer from the movements of small floating bodies, but upward slanting currents of small area might easily escape observation.

It is obvious that upward currents over a plain, caused either by variations in the velocity of the wind or by the unstable state of the atmosphere, must be almost insensible near the ground, and could not attain their full strength under a considerable height. This accounts for the fact that over plains birds do not begin to soar at less than about 200 feet. If soaring were possible in a uniform horizontal current, they would save themselves the muscular effort of rising 200 feet and over by the active use of the wings, and would begin to soar immediately on leaving the ground, as they do in currents blowing up a slope.

I have often observed gulls with extended motionless wings following a steamer in the same relative position for several minutes. In every case it was clear that they used the current diverted upwards by the hull. Before the upward energy of this current is exhausted, a fast steamer has gone a good many yards, so that a bird is supported at some distance astern. Also an upward current of considerable strength would flow off the mizen sail of a ship sailing near the wind and leaning over.

Christchurch, N.Z. A. C. BAINES.

Rain-making in Florida in the Fifties.

THE article on "Rain-making in Texas" (NATURE, p. 473) recalled to my memory a passage of Dr. Th. Reye's book ("Wirbelstürme, Tornados, &c.," Hanover, 1872), in which (at p. 12 and following) the author in question translates quotations from J. P. Espy's "Second and Third Report on Meteorology, 1851, auf Befehl des Senates der Union gedrukt" (Reye's note at his p. 235; quoting also fourth Report, 1857). The facts related were observed by the surveying officers George and Alexander Mackay. They (in Florida) had at their disposal great quantities of rushes (saw-grass), which they set in flame, and the huge conflagrations were invariably followed by rain.

September 22. G. P.

A Dog Story.

THE following dog story may interest your readers.

As I went to the train one morning, I saw a brown retriever dog coming full speed with a letter in his mouth. He went straight to the mural letter box. The postman had just cleared the box, and was about 20 or 30 yards off when the dog arrived. Seeing him, the sagacious animal went after him, and had the letter transferred to the bag. He then walked home quietly.

Putney, September 23. JOHN BELL.

SOME NOTES ON THE FRANKFORT INTERNATIONAL ELECTRICAL EXHIBITION.[1]

II.

A Page of Modern History.

ELECTRIC transmission of power to great distances bids fair in the near future to change the whole commerce of the world, and yet the history of its development is all comprised within the last fourteen years. In a long paper read in the early part of 1877 before the Institution of Civil Engineers, "On the Transmission of Power to a Distance," the author, Prof. Henry Robinson (now the engineer to various electrical companies), does not even suggest the possibility of employing electricity for this purpose. So that in the discussion Sir William Siemens remarked, "He might also refer to another method of transmitting power to a distance, which did not seem to have occurred to the author, perhaps because it was of recent date, viz. by electric conductors."

A week later, Sir W. Siemens, in his Presidential address to the Iron and Steel Institute, throws out the idea of utilizing the power wasted in the Falls of Niagara; and after referring to the use of high-pressure water mains and quick-working steel ropes for transmitting power over one or two miles, he says, "Time will probably reveal to us effectual means of carrying power to great distances, but I cannot refrain from alluding to one which is, in my opinion, worthy of consideration—namely, the electrical conductor." And he adds, "A copper rod three inches in diameter would be capable of transmitting 1000 horse-power at a distance of, say, thirty miles."

The use of the electric current for the transmission of power over considerable distances was, therefore, fully present in the mind of Sir William Siemens in 1877, but not apparently the employment of the high potential differences which are absolutely necessary to make such a transmission commercially possible For a copper rod of three inches diameter, such as he speaks of, has a cross-section of nearly seven square inches, and could carry some 5000 or 6000 amperes without undue heating. Therefore, even when the problem of transmitting 1000 horse-power over thirty miles was in question, he did not contemplate, apparently, using a pressure of more than about 100 volts.

At the commencement of the following year, 1878, in his Presidential address to the Society of Telegraph Engineers, he refers to his previous statement, and adds, "Experiments have since been made with a view to ascertain the percentage of power that may be utilized at a distance." The result obtained, he says, is that "over 40 per cent. of power expended at the distant place may be recovered"; but Sir William adds, in reference to the 60 per cent. loss, "This amount of loss seems considerable, and would be still greater if the conductor through which the power were transmitted were of great length."

The length of the conductor employed in the above experiment is not given, but its approximate length, as well as what is understood by "great length," may be gathered from the context; for Sir William goes on to consider the problem "of distributing the power of a steam-engine of, say, 100 horse-power to twenty stations within a circle of a mile diameter"; and although the distance to which it is proposed to transmit the power is only one mile, he assumes that the loss is what was found in the above experiment, viz. 60 per cent. He further adds, "The size of the conductor necessary to convey the effect produced at each station need not exceed half an inch in external diameter." Clearly, then, as the power proposed to be transmitted by the half-inch conductor to each station one mile distant was only 5 horse, there was no idea of using

[1] Continued from p. 497.

a potential difference in the transmission higher than that maintained between the terminals of a lamp.

Two wrong notions misled people in those days—the one, that the maximum efficiency of a perfect electromotor could be only 50 per cent.; the other, quoting the remarks of Sir W. Siemens in the discussion of the paper read by Messrs. Higgs and Brittle at the Institution of Civil Engineers somewhat later in the same year 1878, "In order to get the best effect out of a dynamo-electric machine there should be an external resistance not exceeding the resistance of the wire in the machine. Hitherto it had been found not economical to increase the resistance in the machine to more than one ohm; otherwise there was a loss of current through the heating of the coil. If, therefore, there was a machine with one ohm resistance, there ought to be a conductor transmitting the power either to the light or the electromagnetic engine not exceeding one ohm." He then goes on to consider that as the conductor is lengthened its cross-section must be increased in proportion to keep the resistance constant at one ohm; and he arrives at a result quite new at the time, viz. that if the number of dynamos *in parallel* were increased in proportion to the length and cross-section of the line, "it was no dearer to transmit electromotive force to the greater than to the smaller distance."

Sir William Thomson grasps at once the novelty and importance of this idea, and renders it even more important by proposing to put all the dynamos *in series* at one end of the line, and all the lamps in series at the other. But it would still appear that even 40 per cent. efficiency for transmision over a considerable distance could only be attained when "there were a sufficient number of lamps" to make it necessary to use *many* dynamos in parallel in accordance with Siemens's proposal, or, *many* dynamos in series in accordance with Thomson's modification of Siemens's proposal.

In 1879, the electric transmission of power was still such a *terra incognita* that the largest firm of electrical engineers in Europe could not be induced to tender for transmitting power over ten miles in India.

At the British Association lecture in the autumn of 1879, Prof. Ayrton exposed the fallacy of assuming that 50 per cent. was the maximum efficiency theoretically obtainable with an electromotor. He further proposed that, instead of employing many dynamos at one end of the line and many lamps at the other, there should be used a single dynamo and a single motor, with much wire on each; that the high potential of the line necessary for economical transmission of power should be maintained by running both dynamo and motor much faster than hitherto; and that both dynamo and motor should be separately excited. Although not wholly free from the prevailing idea of that day—that electric transmission of power over long distances would only be commercially possible when a very large amount of power had to be transmitted—he says, after discussing the subject, "So now we may conclude that the most efficient way to transfer energy electrically is to use a generator producing a high electromotive force and a motor producing a return high electromotive force; and by so doing the waste of power in the transmission ought, I consider, to be able to be diminished with our best existing dynamo-electric machines to about 30 per cent."

This was perhaps the first time that it had been even suggested that the efficiency in electric transmission of power could be more than 50 per cent.

Further, the lecturer proposed to use in all cases this high E.M.F. motor, whether the received power were required for motive purposes, for light, or for electroplating; and, as experimentally shown in the lecture, to generate the current locally in the two latter cases by using the motor to drive a suitable dynamo, thus giving the first illustration of the employment of an electric transformer in the actual transmission of power to a distance.

Two years later, viz. in 1881, the old mistaken notion, that it was only 50 per cent. of the power given to a dynamo that could be returned by the motor, was again propounded during a discussion at the Society of Arts; and the Chairman, Sir W. Siemens, when correcting the speaker's error, added, "Experiments of undoubted accuracy had shown that you could obtain 60 or 70 per cent."

In this year two very important propositions were put forward—the one, by Sir W. Thomson, at the semi-centenary meeting of the British Association, that, in the electric transmission of power, the small current of high potential difference should be employed at the receiving end of the line to charge a large number of accumulators in series, the accumulators being subsequently discharged in parallel for supplying light or power to a town; the other, by MM. Deprez and Carpentier, to use one alternate current transformer at the sending end to raise the electric pressure, and another transformer at the receiving end to lower it down again, the arrangement being symbolically shown in Fig. 1.

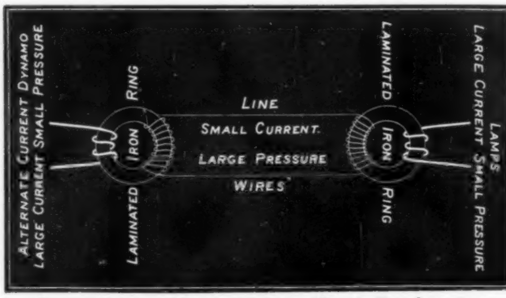


Fig. 1.—Deprez and Carpentier's Plan of Double Transformation.

The great advantage of this combination is, that the pressure along the line may be very high, and the line therefore composed of only thin wire, whereas the pressure between the leads from the generating dynamo at the transmitting end, as well as the pressure between the lamp mains at the receiving end of the line, may be as low as if the dynamo and lamps were close together.

In the experiments, however, made in the following year, 1882, to transmit power from Miesbach to Munich, along thirty-five miles of iron telegraph wire 0·18 inch in diameter, the current going by one wire and returning by another, M. Deprez did not employ his double transforming arrangement described above, probably because alternate current motors were then quite untried practically. But, instead, he used a direct current dynamo generating a potential difference of some 1500 volts, the current from which set in motion a direct current motor, wound to stand a similar high pressure, placed at the other end of the telegraph line.

The experiments were attended with various breakdowns of the dynamo, which was probably constructed on the usual string-and-glue fashion of those days; and finally, after repairs had been effected, the power given out by the motor at Munich was only a fraction of 1 horse, with a commercial efficiency of about one-third

It was, therefore, decided to repeat the experiments the next year, 1883, with machines constructed more solidly, and for the convenience of the jury the dynamo and motor were placed close together in the workshops of the Northern Railway near Paris, one terminal of each being connected by a short wire, and the other terminals by a telegraph wire 0·157 inch thick going from Paris to Bourget and back again, a distance of 18,133 yards. The power used in driving the dynamo was towards the end of this second set of experiments about 10⅓ horse, and the power given out by the motor about 3⅓ horse, the

potential difference at the dynamo terminals being some 1850 volts.

The arrangement of the machines was very bitterly criticized: some pronounced the result a great success; others that the whole thing was a fraud, that the power did not go from the dynamo at Paris to Bourget and back again, but that, owing to leakage from one of the telegraph lines to the other, the actual distance over which the power was transmitted was far less than the distance stated.

The next experiments were made with the same machines rewound and improved in insulation. They were now employed to transmit power over 8¾ miles, from Vizille to Grenoble, a pair of silicium bronze wires 0·079 inch in diameter being used to connect the dynamo and motor. A difference of potential of about 3000 volts was employed, and 7 horse-power was given off by the motor with a commercial efficiency of 62 per cent.

This experiment of transmitting power from Vizille to Grenoble in 1883 was distinctly successful, and constituted a great advance on anything in electric transmission that had been attempted before. It is interesting, for example, to compare it with the transmission from Hirschau to Munich by Mr. Schuckert in 1882, and which was regarded as very striking at the time it was carried out.

Transmission of Power.

	1882. Hirschau to Munich.		1883. Vizille to Grenoble.
Distance in miles ...	3½	...	8¾
Diameter of conducting wire in inches ...	0·18	...	0·079
Horse-power delivered by electromotor ...	5·8	...	7
Commercial efficiency of the transmission ..	36	...	62
Potential difference at terminals of dynamo in volts	700	...	3000

Comparing, then, the Vizille transmission of 1883 with the Hirschau transmission of 1882, we see that the distance was twice as great, the cross-section of the wire less than one-quarter, the power somewhat greater, and the efficiency nearly twice as great; this great improvement being effected by using a pressure of 3000 instead of 700 volts.

But with 3000 volts the limit of constructing the commutator of an ordinary direct current dynamo or motor is reached—a fact which was not appreciated by M. Deprez. For when it was decided somewhat later to try and transmit 200 horse-power through 35 miles of copper wire 0·2 inch in diameter, stretched on telegraph poles between Creil and Paris, by using a pressure of 6000 or more volts, the same system of *direct* current dynamo and motor, that had been employed by M. Deprez in his previous transmissions, was resorted to. The result was that the 200 horse-power had to be reduced to 100, and the dynamo and motor were burnt up time after time.

Eventually, after the expenditure of a very large sum of money, spent in several rewindings of the machines, &c., M. Deprez succeeded in 1886 in obtaining from the shaft of the motor at Paris 52 horse-power, this being 45 per cent. of the power spent in driving the dynamo at Creil. The power delivered at Paris was distributed by coupling a low potential difference dynamo to this motor, and using the current developed by this dynamo for driving various smaller motors, so that the power actually delivered to the pumps, &c., was somewhat less than the 52 horse stated above.

In the use of a dynamo and motor each with a high resistance armature and a low resistance field magnet, the fields being produced by separate excitation, and in the employment of a motor-dynamo for utilizing the received power, M. Deprez expressed his approval of the very plan proposed by Profs. Ayrton and Perry in 1879 for "sending by even quite a fine wire a small current," and so obtaining "an economic arrangement for the transmission of power."

This experiment, although very costly, had considerable interest, in showing that as much as 52 horse-power could be actually delivered at the end of thirty-five miles of copper wire 0·2 inch thick, and that a pressure of 6000 volts could be practically employed with a lead covered insulated conductor. But probably the most important lesson learned from it was, that when the distance over which power had to be transmitted was so great that a pressure of 6000 volts became necessary to obtain economy in the conducting wire, an alternating and not a direct current ought to be used.

While these various experiments of M. Deprez with direct currents were being carried out, the transmission of power by means of alternating currents had been progressing in the face of considerable opposition. The exhibition at the Aquarium, Westminster, in the spring of 1883, will probably be chiefly remembered from its being there that Messrs. Gaulard and Gibbs showed what they called a "secondary generator," which was simply an improved form of Ruhmkorff induction coil, without the ordinary vibrating make and break. A current from an alternating dynamo was sent round one of the coils, and to the terminals of the other were attached lamps, the brightness of which could be varied by pulling out the iron core of the induction coil more or less, as is done with medical coils to alter the strength of the shocking current.

Nobody thought much of the "secondary generator"; it seemed to have no very special use; the iron core felt very hot, so that there would be a new waste of power introduced into electric lighting by the use of secondary generators. Besides, the electricians saw that Messrs. Gaulard and Gibbs were employing methods and apparatus for measuring the power which must give totally erroneous results when used with alternating currents; and so, forgetful of the fact that invention is frequently quite ignorant of the language of the text-book, they decided that there was nothing in it.

But Messrs. Gaulard and Gibbs believed in their secondary generator, whatever electricians and the technical press might say; they put them at the Notting Hill Gate, Edgware Road, Gower Street, King's Cross, and Aldgate stations of the Metropolitan Railway, joined the fine wire coils of all the generators *in series* with one another, and sent a small alternating current through the whole circuit from a dynamo placed at Edgware Road. Lamps of different kinds attached to the thick wire coils of each of the generators at the five railway stations burned steadily and brightly; an alternate current motor, even, which was put at one of the stations, revolved rapidly: but what a great waste of power there must be in all this unnecessary transformation, said the learned.

Well, in the spring of the next year, 1884, Dr. J. Hopkinson tested the efficiency of these secondary generators on the Metropolitan Railway, and, to the surprise of nearly everyone, it came out close on 90 per cent.

In the autumn of the same year, in connection with the Exhibition at Turin, power was transmitted to Lanzo, twenty-five miles away, by means of a bare overhead wire rather less than one-quarter of an inch in thickness, and, by means of Gaulard and Gibbs's secondary generators, the power was distributed at Lanzo and elsewhere along the route, for lighting incandescent and arc lamps. The jury reported that the efficiency of the transformers was 89 per cent., the whole distribution strikingly successful, and a prize of 10,000 francs was awarded to Messrs. Gaulard and Gibbs by the Italian Government.

No electromotors, however, appear to have been driven by the transmitted power, for, even in 1884, alter-

nating current electromotors were still comparatively untried.

Tests of a secondary generator were next undertaken in 1885 by Prof. Galileo Ferraris, of Turin, who found the efficiency at full load to be no less than 97 per cent.,—a value even higher than that previously published. This investigation is the more memorable, in that it led Prof. Ferraris to take up the mathematical and experimental investigation of alternating currents, resulting in the discovery and construction of the self starting alternate current motor in 1885, and to extensions of considerable practical importance in our knowledge of the action of secondary generators, now called *transformers*. And so one of the chief lions this year at the Frankfort Exhibition was Prof. Ferraris. W. E. A.

(*To be continued.*)

THE GIRAFFE AND ITS ALLIES.

ALTHOUGH coming within that well-defined group of ruminants known as the Pecora, the Giraffe (the sole existing representative of the genus *Giraffa*) stands markedly alone among the mammals of the present epoch; although, on the whole, its nearest living relations appear to be the deer (*Cervidæ*). Moreover, not only is the giraffe now isolated from all other ruminants in respect of its structure, but it is also exclusively confined to that part of the African continent which constitutes the Ethiopian region of distributionists. When, however, we turn to the records of past epochs of the earth's history, we find that both the structural and distributional isolation of the giraffe are but features of the present condition of things. Thus, in regard to its distribution, we find that in the Pliocene epoch giraffes were abundant in Greece, Persia, India, and China; and we may therefore fairly assume that they were once spread over the greater part of the Palæarctic and Oriental regions. Then, again, with regard to their allies, the researches of palæontologists have been gradually bringing to light remains of several large extinct ruminants from various regions, which are more or less nearly related to the giraffe, but whose affinities appear to be so complex and so difficult to decipher, that not only do they remove the stigma of isolation from that animal, but even render it well-nigh impossible to give a definition of the group of more or less giraffe-like animals, by which it may be distinguished on the one hand from the deer (*Cervidæ*), and on the other from the antelopes (*Bovidæ*). Since an interesting account of a new extinct Giraffoid from the Pliocene deposits of Maragha in Persia has been recently given by Messrs. Rodler and Weithofer in the *Denkschriften* of the Vienna Academy, the present time is a suitable one to offer a brief *résumé* of the present state of our knowledge of this group of animals, and the different views which have been entertained as to the affinities of some of its members.

Among the chief structural peculiarities of the giraffe, the most noticeable is its great height, which is mainly produced by the excessive length of the neck and limbs. The fore-limbs are, moreover, longer than the hind ones, as is well shown by the circumstance that the radius, or main bone of the fore-leg, is longer than the tibia in the hind-leg; whereas, in other living ruminants the reverse condition obtains. The skull is more like that of the deer than of any other existing ruminants, this being shown by its general contour, and also by the presence of the large unossified space below the eye, which completely separates the lachrymal from the nasal bone; a condition but very rarely met with in the *Bovidæ*, although found in the skull of the water-buck. Then, again, the skull resembles that of the deer in the great elongation of the portion situated behind the eyes, *i.e.* the parietal region. The bony processes arising from the skull between the occiput and the eyes, and clothed in the living animal with skin, are not strictly comparable either with the antlers of the deer or the horn cores of the antelopes; in the young condition they are separate from the bones of the skull, with which, however, they unite as age advances. The whole of the frontal and nasal region is much swollen and inflated by the development of air-cells between the inner and outer layers of bone; and at the junction of the frontal and nasal bones there is a large oval hillock-like protuberance in the middle line, which is sometimes termed a third horn. This excessive inflation of the region of the face makes the appearance of this part of the skull very different from that of the deer, in which it is much flattened. The grinding or molar teeth of the giraffe are remarkable for the peculiar roughness of their external coating of enamel, and also for their broad and low crowns, which in the upper jaw lack the internal additional column occuring in those of most deer and many antelopes. These teeth are, however, more like those of the deer than those of other ruminants, although they can be distinguished at a glance from all others except the larger ones of the under-mentioned fossil forms.

Since a good deal depends on the similarity between the structure of the molar teeth of the giraffe and those of the extinct ruminants in question, it may be well to observe that the characters of the molar teeth among all the ruminants are of great importance in classification. Thus, these teeth in all the deer, although varying to a certain extent in the relative height of their crowns, present the same general structure, those of the upper jaw being comparatively short and broad, with a large internal additional column. Then, again, in the *Bovidæ* we may notice that each of the several groups into which the antelopes are divided, as well as the goats and sheep and the oxen, are severally distinguished by the characters of their molar teeth, and that, although the teeth of one group may approximate more or less closely to that of another, we do not find any instances where one member of a group possesses teeth of a totally different type from those of the other representatives of the same group. These facts strongly indicate that, when we meet with fossil ruminants having molar teeth of the very peculiar type met with in the giraffe, we shall be justified in considering that there must be a certain amount of relationship between the owners of such teeth.

Another marked peculiarity of the giraffe is that the humerus has a double groove for the biceps muscle, instead of the single one found in ordinary ruminants. In regard to its soft parts, the giraffe resembles the deer in the usual absence of the gall-bladder, although its reproductive organs are constructed more on the Bovine type.

With these preliminary remarks on some of the structural peculiarities of the giraffe, we may proceed to the consideration of its fossil allies. The genus which probably comes nearest to the giraffe is the imperfectly known *Vishnutherium*, founded upon part of a lower jaw from the Pliocene of Burma, but to which have been referred some upper molars and bones from the corresponding beds of the Punjab. This animal must have been considerably larger than the giraffe, and the upper molars are remarkable for the great flatness of the outer surfaces of their external columns, in which respect they come nearer to the corresponding teeth of the elk than do those of any other members of the group. The posterior cannon-bone, or metatarsus, assigned to this genus, although relatively much shorter than that of the giraffe, is more elongated and giraffe-like than the corresponding bone of any other fossil genus in which this part of the skeleton has been described. The cervical vertebræ are also more elongated and giraffe-like than those of any of the under-mentioned genera. It will of course be immaterial if these bones prove to belong to a genus distinct from *Vishnutherium*; their interest lying in the

circumstance that they indicate the existence of an animal to a great extent intermediate between the giraffe and the following genus.

The genus *Helladotherium* was established upon the remains of a large giraffe-like ruminant from the Pikermi beds of Greece, to which a skull from the Indian Siwaliks, which had been previously regarded as referable to the female of *Sivatherium*, proved to belong. The Helladothere, of which the entire skeleton is known, was a hornless animal, of larger size than the giraffe, but with much shorter and stouter neck and limbs. The skull approximates in many respects to that of the giraffe, having the same long parietal region, but with a minor development of cells in the frontals, and the important difference that there is no unossified space below the eye. The limbs agree with those of the giraffe in the great relative length of the anterior pair, as is shown by the radius being considerably longer than the tibia. That the Helladothere was not the female of the Sivathere seems to be evident from the absence in the Pikermi beds of the antler-like cranial appendages of the latter, which are comparatively common in the Indian Siwaliks. The intimate affinity existing between the Helladothere and the giraffe has been admitted by all who have written on the subject.

The animal recently described by Messrs. Rodler and Weithofer from the Persian Pliocene, for which the hybrid name *Alcicephalus* has been proposed, tends to connect the Helladothere with the deer, and more especially the elk. Thus, in the first place, the front and hind limbs are approximately equal, the length of the radius and ulna being nearly the same. Then, again, from the total absence of air-cells in the frontal region of the skull, the middle of the face is nearly flat, and the orbits have their frontal borders in the plane of the face, instead of considerably below it, as in the Helladothere, and still more so in the giraffe. There is, however, no unossified space in front of the eye; although the whole contour of the skull is strikingly elk-like.

The conclusion to be drawn from these hornless forms appears to be that they serve to connect the giraffe with less aberrant ruminants, and more especially the *Cervidæ*, and also that the unossified vacuity in the skull of the giraffe is probably an acquired feature, since it is absent both in the extinct giraffoid genera, and in the earliest deer, like the Miocene *Amphitragulus*. Both giraffes and deer may, therefore, probably have had a common ancestor more or less closely allied to the lower Miocene genus *Gelocus*.

Leaving now these hornless forms, as to the affinities of which there has been no dispute, we have to turn our attention to another group provided with cranial appendages of very curious and still imperfectly understood structure, in regard to whose relationship exceedingly different views have been entertained. This group, so far as we know at present, seems to be confined to the Pliocene of India and Persia, being represented in the former area by the gigantic *Sivatherium*, *Bramatherium*, and *Hydaspitherium*, and in the latter by the much smaller *Urmiatherium*. In all these animals the skull is characterized by the extreme shortness of the parietal region, and the position of the horns or antlers immediately over the occiput; the elevated facial profile thus produced being in very striking contrast to the straight one of the deer. In *Bramatherium* and *Hydaspitherium* the cranial appendages rise from a massive common base, and the latter genus is distinguished from all the others by the presence of an unossified space below the eye, corresponding to that of the giraffe. Their molar teeth are very similar to those of the Helladothere. In the Sivathere, on the other hand, there is one pair of large branching and palmated cranial appendages rising from separate bases immediately above the occiput; and in addition to these a pair of much smaller conical ones placed immediately over the orbits. In general appearance the large palmated appendages are more like the antlers of the elk than those of any other existing ruminants; but the absence of a "burr" at their base indicates that they were not deciduous, while the deep arterial grooves on their surface suggest that they were clothed either with skin or with a horny substance. The molar teeth conform to those of the giraffe—and to a less degree the deer—having the same rugose enamel; but the ridges on the outer surfaces of those of the upper jaw are more developed than in the other extinct genera. A peculiarly giraffe-like and cervine feature in these upper teeth is the extension of the anterior extremity of the anterior crescent far towards the outer side of the crown. Lastly, the humerus of the Sivathere resembles that of the giraffe in the presence of a double groove for the biceps muscle; while the form of the terminal bones of the feet is almost identical in the two animals. In the small Persian *Urmiatherium*, which is known only by the hinder portion of the skull, it appears that the cranial appendages consisted of a pair of unbranched, somewhat compressed, and upright processes rising immediately above the occiput.

With regard to the affinities of this group, it has been argued that the shortness of the parietal region of the skull, and the position of the cranial appendages immediately above the occiput, indicate affinity with certain African antelopes, such as the Sassabi and its kindred (*Alcelaphus*). In that group of antelopes it is, however, perfectly clear that the features in question are acquired ones; the allied Blessbok scarcely possessing them in any degree. Again, the straightness of the cranial axis in the skull of Waller's gazelle (*Gazella walleri*) shows that the arching of this axis, which is so characteristic of most antelopes, is likewise a feature specially acquired among that group of animals. Moreover, apart from this evidence, no one who thinks for a moment on the subject can believe that the Sassabi, with its narrow sheep-like molars and true horns, and the Sivathere, with its broad giraffe-like molars and cranial appendages, which are neither true horns nor true antlers, can be anything approaching to first cousins; and yet if they are not so, it is perfectly evident that the similarity in the structure of their skulls must have been independently acquired. It is therefore abundantly clear that no arguments based on these resemblances will hold water; the true explanation probably being that the superficial similarity of their skulls is solely connected with the support of cranial appendages having a similar position in both groups.

It follows from this that, if a type of skull with a short parietal region, a curved basal axis, and horns placed immediately over the occiput, has been independently developed among the antelopes from a type of skull with a long parietal region, a straight basal axis, and horns placed over the orbits, there is no conceivable reason why a similar line of development should not have taken place among giraffe-like animals. Taking, therefore, into consideration that the Sivathere and its allies have molar teeth like those of the giraffe, that their cranial appendages could be derived from those of the latter by special modification and development better than from those of any other group, that their humerus has a double bicipital groove, that the terminal phalangeals of their feet are giraffe-like, and that the proportions of their limbs are only a step beyond those obtaining in the admittedly giraffoid Helladothere, the evidence in favour of regarding these animals as greatly modified Giraffoids is so strong as to be almost a certainty. Indeed, it appears most probable that we ought to regard the Sivathere and its allies as holding a somewhat analogous position among the Giraffoids to that occupied among the antelopes by the Sassabi and its cousins.

The writer has purposely refrained from making any reference to the large unossified suborbital vacuity in the skull of the Hydaspithere, as reasons have already been

given for regarding that feature as an acquired one. If, however, that view be incorrect, the presence of this vacuity at once stultifies the statement that the Sivathere can have no kinship with the giraffe and the deer, on account of the absence of a similar vacuity; and its presence, so far as it goes, is also another argument against the Sassabi theory.

The last representative of the Giraffoid animals that we have to mention is the recently discovered *Samotherium*, from the Pliocene of Samos, a figure of the skull of which appeared in NATURE, illustrating an article on the extinct mammals of those deposits. In this animal, the elongated form and straight profile characteristic of the skull of the Giraffe are retained; and the teeth are almost indistinguishable from those of the latter. There is, however, no development of air-cells in the bones of the frontal region, so that the upper border of the orbit is approximated to the plane of the face; and the cranial appendages take the form of upright compressed processes rising immediately over the orbits. These appendages, which appear to have been inseparable from the bones of the forehead, are, indeed, very similar, both in form and position, to the horn-cores of certain extinct antelopes, but we are, of course, unacquainted with the nature of their covering. If, however, as seems to be undoubtedly the case, the Samothere is a Giraffoid, it would seem that we must here again regard this superficial resemblance to the antelopes as one independently acquired.

Finally, if the views expressed above are anywhere near the truth, it would appear that, in the Pliocene epoch, Giraffoid animals played a very important *rôle* among the ruminants, and that they have undergone modifications and developments fully as marked as those which we observe among the antelopes at the present day. Whether the circumstance that none of them, except the giraffe (which is obviously an animal incapable of further modification), appears to have obtained an entrance into Africa has been the chief reason why only a single representative of the group has survived to our own times may be a fair subject of conjecture, since after the Pliocene epoch both India and Europe seem to have been unsuited to the maintenance of many forms of large Artiodactyle Ungulates, as is proved by the disappearance from those regions of the hippopotamus, the giraffe, and a number of antelopes of African type. R. L.

PHOTOGRAPHIC MAGNITUDES OF STARS.

THE character of the image of a star photographed on a sensitized film; the relation between the intensity of the light photographed and the blackened disk produced; the influence of the time of exposure on the image—are questions now receiving much attention. For this reason, Dr. Scheiner's contribution to the subject, embracing, as it does, the latest results of the Potsdam Observatory, is especially welcome; but these results will not be accepted without great reserve, contravening, as they do, a theory, or at least an assertion, that has been very generally accepted, viz. that increasing the intensity of light is exactly equivalent to increasing the time of photographic exposure. A consequence of such a law would be that an additional magnitude would be impressed on the film by increasing the time of exposure two and a half times the length.

Such a law cannot be rigorously exact, and its stoutest supporters have been careful to confine its application "within limits." But Dr. Scheiner's contention is that, owing to the complex character of the disk produced on the film, such a principle is a very unsafe guide, either as a rule for the determination of the feeblest magnitude impressed on the negative, or as offering a satisfactory explanation of the growth of the diameter or area.

In the first place, there is evidence of want of uniformity of actinic action throughout the whole extent of the stellar disk. A mean intensity (i) may be assumed at a certain distance (r) from the centre of the image, where the intensity is I. This centre will not be a geometrical point, but, owing to atmospheric and other disturbances, will occupy a small area of radius (ρ). The intensity (i) at distance (r) will depend materially on the increase of the area (ρ), which may be represented by $\psi(\rho)$. Consequently, the simplest expression for $i = \mathrm{I}\psi(\rho)e^{ar}$, where a is the coefficient of absorption of the sensitive film. On comparing two stellar disks, formed on the same emulsion, and treated by the same developer, this expression becomes

$$\frac{t_0}{t_1} = \frac{\mathrm{I}_1\,\psi(\rho_1)}{\mathrm{I}_0\,\psi(\rho_0)}e^{a(r_1-r_0)},$$

and, if the disks be on the same plate, $\rho_1 = \rho_0$ and $t_1 = t_0$, so that the formula can be simplified to

$$a(r_0 - r_1) = \log\frac{\mathrm{I}_1}{\mathrm{I}_0} = \frac{0\cdot4}{\text{mod.}}(m_1 - m_0)$$

In order to derive the relation between diameters and exposure, put $\mathrm{I}_0 = \mathrm{I}_1$, and then

$$\log\frac{t_0}{t_1} = a(r_1 - r_0).$$

It is not likely that such an expression has any other value than to serve as a convenient formula for interpolation. The variable character of a under different conditions, but always depending on the time of exposure, is shown by the following table:—

Exposure. m. s.	Instrument.	a.	Instrument.	a.
1 0	Reflector	4·99	5-in. refractor	4·12
2 0	,,	4·57	,,	5·09
4 0	,,	4·67	,,	5·47
8 0	,,	4·89	,,	5·89
16 0	,,	5·39	,,	7·51
0 24	13-in. refractor	3·18	13-in. refractor	2·67
1 0	,,	3·16	,,	2·20
2 30	,,	3·33	,,	2·48
6 15	,,	3·33	,,	3·00
15 38	,,	4·48	,,	—

Another well-known formula in which magnitude is made to depend on diameter is $m = a - b \log \mathrm{D}$, and in this case b is shown, notwithstanding Dr. Charlier's results to the contrary, to be a function of the time of exposure. The results are as follows:—

Time of exposure. h. m.	b Charlier.	Time of exposure. m. s.	b Scheiner.
0 13	6·719	0 24	5·17
1 30	6·779	1 0	6·35
2 0	6·683	2 30	7·06
3 0	6·814	6 15	8·08

The disagreement is conspicuous, but the explanation offered by Dr. Scheiner is scarcely satisfactory. He would ascribe the constancy in the value of b, found by Dr. Charlier, to the fact that in his experiments there is always a large absolute value of the time coefficient. It will, however, be observed that the ratio between Dr. Charlier's extreme exposures is not greatly different from that which obtains in Dr. Scheiner's experiments.

If it be admitted that the product of intensity by the time is *not* a constant quantity, it becomes a matter of great practical importance to determine what is gained on a photographic plate by prolonged exposure. This question forms the real investigation of Dr. Scheiner's two papers, and though some of his results may be questioned, yet the general issue is so grave and disquieting that it may not be utterly ignored. Passing over the details of his method of examination, and the precautions taken to insure accurate results, for which the reputation

of the Potsdam Observatory is a sufficient guarantee, Dr. Scheiner presents the following table, in which is exhibited the faintest magnitude which, under certain varied circumstances, can be detected on a photographic plate:—

Time of exposure.	Faintest magnitude.			
m. s.	Plate I.	Plate II.	Plate III.	Plate IV.
0 24	9·0	6·4	7·7	8·2
1 0	9·4	7·25	8·3	8·75
2 30	9·9	7·7	8·55	9·3
6 15	10·6	8·45	9·3	9·65
15 38	—	8·85	9·7	—

It will be noticed that while each successive exposure is 2·5 that of the preceding, the corresponding gain in light is considerably less than one magnitude. From each of the four plates the gain is as follows:—

	Gain in mag.
Plate I.	0·53
,, II.	0·61
,, III.	0·50
,, IV.	0·48

The mean is 0·53—that is to say, instead of one magnitude being gained by continued exposure through each successive interval, the actual gain is only half a magnitude. The exception that might be taken to these experiments is, that the detection of the feeblest stars on a plate is a matter of doubt and great practical difficulty. Dr. Scheiner has, however, availed himself of a second test by counting the stars on a plate after various exposures. With this view two plates were taken of the region round ϵ Orionis, one with an exposure of one hour, the other with eight hours' exposure. Therefore, if 2·5 times the exposure produced stars a magnitude fainter, there ought to be a gain of more than two magnitudes on the second plate, and it may be assumed that the number of stars impressed would follow the known law. On the one-hour plate were found 1174 stars, on the eight-hour 5689. There ought to have been on the long-exposed plate over 10,000 stars, so that roughly speaking only one-half of the stars given by the law were photographed. Further, Argelander has catalogued within this area 125 stars, and therefore it might have been anticipated from the law of increase that some 10,000 stars would have been visible on the one-hour plate.

This margin is too great to be readily explained away. Of course, there is the same difficulty in perceiving the minute dots that represent the faintest stars as in the former case, and further, it is possible that the law of average increase of the number of stars did not hold in this particular part of the sky. It is not to be expected that a law, which applies with more or less accuracy *on the average* to the whole of the sky, is necessarily fulfilled on any small portion, such as the ten-thousandth part. If the stars are not in the heavens, they cannot be photographed. Evidently, it would be unlikely that on every thousandth part of *that* plate would be found the thousandth part of the total number of stars impressed.

But allowing for errors of exaggeration and observation, the result is very interesting, and not a little alarming as implying that photography is not so powerful an engine as was at first anticipated, and that, to accomplish the full hope of all that was expected of it, longer exposure and consequently a greater expenditure of time will be needed. Dr. Scheiner gives a little table, which shows that if a star of the 9·5 mag. be registered in 24 seconds, then in 190 minutes a star of the 16·5 mag. will be photographed, supposing a whole magnitude to be gained by successively multiplying the exposure by 2·5. But if the gain be only 0·5 in this interval, then the faintest star impressed will be only 13·0 mag., even after this long exposure. If 0·6 of a mag. be the rate of increase, then the 13·6 mag. will be seen; if 0·7, then 14·4 mag. The truth will probably be found near this latter limit.

NOTES.

THE second International Folk Lore Congress meets at the rooms of the Society of Antiquaries this afternoon, when an address will be delivered by Mr. Andrew Lang, the President. Three subjects are to be considered—folk tales, mythology, and institutions and customs. To each of these subjects a day will be devoted. The proceedings will be brought to an end on Wednesday morning next.

THE Iron and Steel Institute will meet at the Woolwich Arsenal on Tuesday next. The members are to be conducted over the manufacturing departments at the Arsenal, and will see quick-firing and machine guns in practice. On the following day the Institute will conclude its meeting at the Institution of Civil Engineers.

THE third biennial session of the International Statistical Congress was opened at Vienna, on Monday, by Baron Gautsch, the Austrian Minister of Public Instruction. An address was delivered by Sir Rawson Rawson, the President.

THE seventeenth Annual Congress of the Sanitary Association of Scotland was held in Edinburgh last week. Dr. Farquharson, M.P., President of the Congress, delivered an address "On a Model Hygienic State, or a Glance at the Sanitation of the Future." In the course of his remarks he urged the necessity for more organized attention being given in Parliament to hygienic matters, and advocated the appointment of a Minister of Public Health.

THE Harveian Oration will be delivered at the Royal College of Physicians, by Dr. W. H. Dickinson, at the Royal College of Physicians, on Monday, October 19, at 4 o'clock.

WE referred last week to the death of Prof. W. Ferrel. He was born on January 29, 1817, and since the foundation of the *American Meteorological Journal* he was a frequent contributor to that paper, from which we take most of the following details of his life. During his boyhood he was kept rather closely at work on his father's farm, and with the first money he earned, he bought a copy of Park's "Arithmetic." Having also a liking for astronomical studies, he used to draw a number of diagrams upon the doors of his father's farm, describing circles with the prongs of a pitchfork. In 1839, he entered one of the Colleges in Pennsylvania, and graduated at Bethany College in 1844. In 1857, he became an assistant in the office of the "American Ephemeris and Nautical Almanac," and subsequently entered the U.S. Coast Survey and the Signal Office, from which last he retired in 1886. He was elected a member of the National Academy of Sciences in 1868. Ferrel is described as an extremely diffident man, and he never once sought position; every official position that he occupied having been offered to him. His first paper bearing directly on meteorology was published in 1856, with reference to the deflective effects of the earth's rotation upon the motions of the atmosphere; and this paper, which has done much towards establishing meteorology on a scientific basis, was subsequently revised and reprinted as one of the professional papers of the Signal Service, under the title "Motions of Fluids and Solids on the Earth's Surface." In this treatise he proposed a complete analytical investigation of the general motions of the fluids surrounding the earth. These papers received considerable attention and discussion soon after publication, especially in France; in America and England they were overlooked until recent years, but they are now recognized as fundamental propositions in the study of meteorology. He also wrote various articles on the tides, which are of equal significance with those on the motions of the atmosphere, and he constructed a "maxima and minima tide-predicting machine," which is now in use at the Coast Survey Office in Washington. The last of his numerous works upon meteorology was a "Popular Treatise on the Winds," published

in 1889, and reviewed at length in our columns (vol. xli. p. 124). In this work he has explained at length, and with great clearness, many points which in his other writings have been too mathematical to allow of their being generally understood.

WE have already recorded with regret that Miss E. A. Ormerod has considered it desirable to resign her post of Consulting Entomologist of the Royal Agricultural Society, which she has occupied for about nine years, having been appointed in 1882. We understand that her reasons for resignation are partly on account of health, as in wet and cold weather she cannot take the requisite journeys to attend Committees without risk; partly on account of claims made of power of Council to direct her to render service in reporting elsewhere, and claims also made as to use of information in her possession beyond what the terms of her engagement granted. These claims, we understand, have been withdrawn, but Miss Ormerod considers she can work more efficiently when freed from the anxieties and possible ties which public office necessarily brings with it. Miss Ormerod's agricultural entomological work, as shown by her annual reports, has now been going on steadily for at least fourteen years, having been begun several years before she was elected to the staff of the Royal Agricultural Society; and this she purposes to continue precisely as before in all respects, whether as regards replies to inquiries, or publication by herself of observations in the form of yearly reports.

IN an article on Hooker's "Icones Plantarum," in our last issue (p. 498) we attributed the plates of the earlier volumes to Sir William Hooker. Sir Joseph Hooker informs us that they are all the work of Mr. W. H. Fitch.

A VALUABLE report, by Mr. A. E. Shipley, on an orange disease in Cyprus, caused by a scale insect, is published in the September number of the *Kew Bulletin*. The disease appears to have been noticed in Cyprus for the last six or eight years. The particular insect to which it is due is *Aspidiotus aurantii*, Maskell, a member of the sub-family *Diaspinæ*, which, with some others, compose the family *Coccidæ*. Mr. Shipley gives an account of the life-history of this insect, and then describes the various methods of dealing with it. The most successful of these methods is the gas treatment, a full description of which, by Mr. Coquillett, is quoted by Mr. Shipley from Bulletin No. 23 of the U.S. Department of Agriculture, Division of Entomology. We may note that Mr. Shipley is anxious to obtain examples of *Coccidæ* which infest plants, and examples of nematode worms parasitic in plants, with the affected parts of their respective hosts.

THE *Kew Bulletin* for September, besides Mr. Shipley's report on orange scale in Cyprus, contains sections on the rediscovery of gutta-percha trees at Singapore, on a new process for recovering some portion of the gutta-percha which is left in the bark of the trees after collection by the ordinary native method, on the fodder plant Tagasaste, and on Kangra buckwheat.

THE *Oesterreichische Botanische Zeitung* for September contains a report of Dr. A. v. Degen's botanical excursion to the island of Samothrace, and of Dr. R. F. Solla's to Southern Istria.

THE fourth number of the first volume of *Contributions from the U.S. National Herbarium*, published under the auspices of the Department of Agriculture at Washington, consists of a description, by Mr. J. N. Rose, of the plants collected by Dr. E. Palmer in 1890 in Western Mexico and Arizona. Forty-five new species are described, and several of these are illustrated by plates. Most of the new species obtained were from the ighbourhood of Alamos, a mining town of about 10,coo inhabitants, situated 180 miles south-east from Guaymas, at an altitude of about 1275 feet, where there are both a dry spring and a rainy autumn flora, very different from one another. Dr. Palmer has again started for a year's exploration of Western Mexico.

SOME valuable and interesting notes on the fertilization of South African and Madagascar flowering plants, by Mr. G. F. Scott Elliot, appear in *Annals of Botany* (vol. v., No. xix., August 1891), and have also been issued separately. They represent much work done during a two years' botanical trip. While travelling, Mr. Elliot found it impossible to make as thorough and complete observations as are really required for a proper comprehension of all the adaptations of a flower to insect visitors; but he tried to collect every insect which he saw visiting flowers, and brought home with him a numbered collection. Most of the forms secured by him had not previously been studied in their native haunts.

THE Transactions of the Liverpool Biological Society for 1891 contain an important paper by Mr. G. Murray on the Distribution of Marine Algæ in space and in time. The author compares the algal flora of three widely separated regions—the Arctic Sea, the West Indian region, and Australia; and shows in a table how many genera and species are common to any two of the regions. The number of known species of seaweeds is given as 259 in the Arctic Sea, 788 in the West Indies, and 1132 in Australia. Only twelve species are common to all three regions, and of these four belong to the Ulveæ.

A GREAT Mining Exhibition is to be opened at Johannesburg next July, and exhibits from all parts of the world are invited.

THE administration report of the Marine Survey of India for the official year 1890–91, by Captain R. F. Hoskyn, has been published. For some time notices had been received from several vessels, which seemed to indicate that the shoals lying off the eastern coast between Ennore and Pulicat were extending seaward. In the early part of 1890, therefore, the *Investigator* proceeded to this neighbourhood, and made a survey of the coast between these two places, carrying the soundings out to the 100-fathom line. The result showed that no material change had taken place in the size or position of the shoals from the time of the previous survey. The work of the season ended on May 7, when the *Investigator* arrived at Bombay. In October last a new season's operations began, and at the time when the report was written (March 9, 1891) the survey of the eastern coast of Hindustan had been completed to lat. 16° 50′ N.

THE report of Dr. A. Alcock, surgeon-naturalist on board the *Investigator*, is one of great interest. It is given as an appendix to that of Captain Hoskyn. We have already referred to Dr. Alcock's account of the general results of his deep-sea work. It may be noted that on November 3, 1890, the deepest haul ever made in Indian seas—1,997 fathoms—was successfully carried out in lat. 9° 34′ N., long. 85° 43′ 15″ E., the bottom being Globigerina ooze with pieces of water-worn pumice, and the bottom-temperature being 35° F. About 2200 fathoms of wire were veered. The following was the entire take:—There were three species of siliceous sponges and numerous detached spicules of *Hyalonema*; a large sea-anemone of a salmon-pink colour, with bright red tentacles; a mutilated specimen of the Brisingoid *Freyella benthophila*, Sladen, a fine new species of *Hyphalaster*, and a small, probably new, species of *Marsipaster* with the nidamental pouches widely open and full of ova; two species of Ophiurids, one of which is *Ophiomastus*; three species of Holothurians including *Echinosoma*; numerous specimens of a long-stalked Ascidian; two specimens of a very large species of *Amphipod*, a blind Crangonid, three species of macrurous Crustaceans, and a small *Scapellum*; a

small Lamellibranch; and a number of empty annelid tubes, some of which were constructed of Foraminifera shells, while others consisted of agglutinated silky (siliceous) threads.

MR. W. L. DALLAS, assistant meteorological reporter to the Government of India, has written a valuable paper on the meteorology and climatology of Northern Afghanistan, the facts having been collected by officers connected with the Afghan Delimitation Commission. Taking the whole of the record into consideration, Mr. Dallas thinks it may safely be maintained that in the great majority of cases the disturbed weather which appears over North-Western India during the winter and spring months is the result of disturbances, which either effect simultaneously the whole region comprising Afghanistan, Baluchistan, and North-Western India, or which have appeared first over Afghanistan and secondly over India, and that these disturbances have seldom originated in India itself or are confined to India.

WE have received from the Meteorological Council their Quarterly Weather Report for July to December 1880, and Monthly Weather Report for May to December 1887. The Quarterly Reports, which commenced with the year 1869, contain, in addition to the monthly and five-daily means of the observations made at the seven observatories, plates of the continuous curves of the self-recording instruments, which have been etched at the Office, and are perhaps the most complete and perfect series of meteorological curves hitherto published, and also a condensed account of the most important meteorological changes of the period. The Quarterly Reports are now discontinued, and the publication of a Monthly Weather Report was undertaken in 1884 in substitution for the Quarterly Report, while the hourly observations and means have been published in a separate volume. This Report contains the results of observations made at a considerable number of stations, together with a chronicle of the weather, and charts showing the average conditions of the various elements. Both the Quarterly and Monthly Weather Reports also contain a number of elaborate discussions of various allied subjects. The Monthly Reports in the form hitherto issued have been modified; and instead of appearing as a separate work, a Monthly Summary of the Weather, on a more concise plan, has been added to the Weekly Weather Report, commencing with the year 1888. With the exception of the years 1881–83 we have therefore a continuous and valuable record of the weather—in addition to such as is afforded by the Daily and Weekly Reports—since 1869, and we believe it is the intention of the Council to connect the gap between the Quarterly and Monthly Reports at an early date, by a discussion of the weather for that period. We shall refer in a future number to the publications which deal with the observations and results at the Stations of the Second Order, which are more particularly of a climatological character, without discussions of current weather.

THE Park Commissioners of Boston, U.S., have set apart three parcels of land for the establishment, by the Boston Society of Natural History, of zoological gardens and aquaria. It is essential that 200,000 dollars should be raised before any attempt can be made to realize the scheme as a whole, but if a third of the amount were subscribed, one of the two proposed aquaria might at once be instituted. An appeal has been made by the Society to the people of Boston for the necessary funds, and it will be strange if it does not meet with a ready and liberal response. The Society is sanguine enough to think that every public-spirited citizen will see in the scheme "an addition to the forces which increase the intelligence of the voter, and thereby tend to make Boston a more desirable place of residence."

STUDENTS of the Ice Age will read with interest a paper by Mr. N. S. Shaler on the antiquity of the last glacial period, submitted to the Boston Society of Natural History, and printed in the latest instalment of the Society's Proceedings. Mr. Shaler differs decidedly from those geologists who suppose that the end of the glacial period is probably not very remote from our own day. One of the strongest of his arguments is derived from the distribution of the vegetation which in America has regained possession, by migration, of the glaciated district. We must conceive, he points out, that as the ice retreated and gradually disappeared from the surface a considerable time elapsed before existing forests attained their organization. He assumes as certain that the black walnut and the pignut hickory, between Western Minnesota and the Atlantic coast, have advanced, on the average, a distance of 400 miles north of the ancient ice front to which their ancestors were driven by the presence of the glacial sheet. For several reasons he believes that the northward progress of these forms must have been due mainly, not to the action of streams or tornadoes, but to the natural spread of the seed from the extremities of boughs, and to the carriage of the seed by rodents. But allowing for every conceivable method of transportation, he argues that a period of ten or even twenty thousand years is wholly inadequate to account for the present distribution of these large-seeded trees. If they occurred only sporadically in the northernmost part of the field they occupy, their implantation might be regarded as due to chance action. The fact, however, that they extend from the Atlantic to Minnesota indicates that the advance was accomplished by causes of a general and continuous nature.

"WATER-BIRDS that live in the Woods" formed the subject of an interesting paper read lately by Mr. G. B. Sennett before the Linnæan Society of New York. About a dozen species were dealt with, the most interesting of them perhaps being the tree ducks (*Dendrocygna autumnalis et fulva*). The former is found in the heaviest timber along the Rio Grande of Texas, at Lomita, and as this river furnishes no sort of food, it adapts itself to circumstances and feeds upon seeds or grain. These ducks will alight upon a stalk of growing corn with the ease of a blackbird, and are quite at home among the lofty trees where they make their nests. They do not resort to the river, which is so cold and muddy, from the melting snows of the mountains whence it flows, that all vegetable and animal life save the garpike is wanting. No ducks of any kind are found upon it. A flock of cormorants, about four miles long and one mile and a half wide, was once seen by Mr. Sennett in Minnesota.

SPARROWS do not seem to lose in New Zealand any of the audacity for which they are famous in Europe. In a paper read some time ago before the New Zealand Institute, and now printed in the Transactions, Mr. T. W. Kirk gives an example of what he calls their "daring and cool impudence." Between Featherston and Martinborough he heard one day a most unusual noise, as though all the small birds in the country had joined in one grand quarrel. Looking up, he saw a large hawk (*C. gouldi*—a carrion-feeder) being buffeted by a flock of sparrows. They kept dashing at him in scores, and from all points at once. The unfortunate hawk was quite powerless; indeed, he seemed to have no heart left, for he did not attempt to retaliate, and his defence was of the feeblest. At last, approaching some scrub, he made a rush indicative of a forlorn hope, gained the shelter, and there remained. Mr. Kirk watched for fully half an hour, but he did not reappear. The sparrows congregated in groups about the bushes, keeping up a constant chattering and noise, evidently on the look-out for the enemy, and congratulating themselves upon having secured a victory.

IF we may judge from the Report of the Department of Agriculture, Victoria, for the year 1889–90, the farmers of that colony are likely to benefit largely by the work of the agricultural authorities. The Department is efficiently organized, and has a thoroughly scientific conception of the nature of its

duties. Mr. D. McAlpine, who has been appointed consulting vegetable pathologist, presents the following summary of the tasks undertaken by his particular section: (1) special investigations concerning the rust of wheat, oats, barley, and other cereals, and, connected with that, the question of rust on various grasses—native and imported; (2) investigations of the life-histories of the various fungus pests, and a knowledge of the best time to cope with them; (3) reports upon diseased specimens sent in from different parts of the colony, and the best known remedies for the palliation or prevention of such diseases; (4) collection of specimens of the various diseases due to fungi, and the subsequent formation of a museum for educational purposes; (5) delivery of lectures in different centres on the fungus pests most prevalent there; (6) preparation of illustrated handbooks, describing the nature of the various diseases and the remedies to be employed where possible; (7) testing various fungicides and the best methods of applying them; (8) visiting different districts in order to find out prevailing and injurious fungi; (9) contributing periodic reports to the official Bulletin of the Department.

In the Proceedings of the Bath Natural History and Antiquarian Field Club (vol. vii. No. 2), Mr. J. F. Mostyn Clarke gives an account of the geological formations exposed in the cuttings of the Bridgwater Railway, the construction of which opened up a continuous line of excavation through the heart of the Polden Hills. Mr. Clarke had charge of the construction of the railway until near the completion of the earthwork, so that he had excellent opportunities for making careful observations. Geologists may be glad to have his description of the strata when the slopes of the cuttings are overgrown.

Messrs. Longmans, Green, and Co. have published the sixth edition of "An Elementary Treatise on the Integral Calculus," by Dr. Benjamin Williamson, F.R.S. In this edition the work has been revised and enlarged.

Messrs. Mitscher and Röstell, 61*a* Jägerstrasse, Berlin, have issued an important list of books which they have for sale. The works relate to the various departments of botany.

Two communications upon the volatile carbonyl compounds of platinum, from Dr. Pullinger, of Tübingen, and Drs. Mylius and Foerster, of Charlottenburg, appear in the last number of the *Berichte*. Since the preparation of the remarkable carbonyl compounds of nickel and iron by Messrs. Mond, Langer, and Quincke, these platinum compounds, discovered by Schutzenberger in the year 1868, have become more interesting, and the two papers now before us add considerably to our knowledge of them. They are compounds containing platinum, chlorine, and carbon monoxide, and Schutzenberger assigned to them the formulæ $PtCl_2CO$, $PtCl_2 . 2CO$, and $2PtCl_2 . 3CO$ respectively. He obtained them by heating spongy platinum to a temperature of 250° C. in a stream first of chlorine and afterwards of carbon monoxide. The volatile, readily fusible, and crystalline sublimate obtained contained a mixture of the three, and he effected a separation by extraction with carbon tetrachloride, in which the three compounds are differently soluble. They are well defined by their melting-points, which are 194°, 142°, and 130° C. respectively. They are decomposed by water with separation of platinum, formation of hydrochloric acid, and evolution of carbon dioxide, and also, in case of the second and third compounds, of carbon monoxide. The most stable of these compounds and the best investigated is the simpler one, $COPtCl_2$. It appears to possess a distinctly basic character, so that it is able to combine with hydrochloric acid to form a compound, $COPtCl_2 . HCl$; this compound is formed in solution when the crystals are dissolved in concentrated hydrochloric acid. The two other compounds are decomposed by hydrochloric acid, losing carbon monoxide and forming the hydrochloride of the first compound. On evaporation of the hydrochloric solution, the first compound is left in needle-shaped crystals. When phosgene gas, $COCl_2$, is passed over the crystals, drops of liquid are formed, which consist of a solution of the compound in liquefied carbonyl chloride. In addition to these compounds, the bromide and iodide corresponding to the compound $COPtCl_2$ have been prepared. When the hydrochloric acid solution of the latter is evaporated on a water-bath in a stream of hydrobromic acid gas, and the resulting compound extracted with benzene, the filtered solution deposits, on cooling, orange-red needles of the bromide, $COPtBr_2$. The bromide has likewise been obtained by Dr. Pullinger, by passing carbon monoxide over heated platinous bromide. Similarly, the iodide has been prepared by evaporating crystals of the chloride with excess of hydriodic acid solution, and treating the residue with warm benzene. The crystals of the iodide, $COPtI_2$, which separate from the benzene solution on cooling, are deep red in colour, with a violet surface reflection. The chloride, bromide, and iodide exhibit a beautifully graduated difference of properties. Thus the chloride is yellow, the bromide orange, and the iodide red in colour. The melting-points are 194°, 181°, and 140° respectively. The chloride is readily, the bromide difficultly, and the iodide not at all volatile. The chloride is strongly hygroscopic, the bromide less so, and the iodide permanent. In addition to these compounds, another has been obtained by Dr. Pullinger, of the composition $PtCl_2 . 2COCl_2$, in the form of non-volatile yellow crystals, readily soluble in water, from which it recrystallizes unchanged. It appears to be the most stable of all these platinum compounds, but is only obtained in very small quantity.

OUR ASTRONOMICAL COLUMN.

Influence of Aberration upon Observations of Solar Prominences.—Some recent observations of the development and movement of solar prominences have led M. Fizeau to consider the influence that the aberration of light may exercise upon them. A note relative to such an inquiry is contained in *Comptes rendus* for September 7. It is well known that, in consequence of aberration, the longitude of the sun, and therefore of the prominences, is diminished by the amount of the constant, 20"·445—an apparent displacement depending upon the earth's orbital velocity. And it results from this that if a prominence is developed in the neighbourhood of the ecliptic, and the luminous matter of which it is composed has a velocity of translation equal to the velocity of the earth in its orbit, its position will suffer a displacement of 20"·445, which may be added to the effect due to the earth's motion, or otherwise, according to the direction of propagation, and thus give rise to corresponding variations in distances from the edge of the sun. As a matter of fact, however, the velocities of prominences are not uniform, and do not commonly attain the required value; nevertheless it seems that the high velocities which have been determined must give rise to apparent movements which depend upon the laws of aberration, and which ought to be taken into account in precise measurements.

Another point touched upon in the communication to which reference has been made is the physical nature of prominences. The simplest hypothesis is that they represent clouds of incandescent hydrogen and other metallic vapours; but M. Fizeau favours the idea that their visibility is the result of the passage of electrical discharges through gaseous material.

New Asteroids.—The 317th asteroid was discovered by Charlois on September 8, and the 318th on September 11.

SOME OF THE POSSIBILITIES OF ECONOMIC BOTANY.[1]

Our Association demands of its President, on his retirement from office, some account of matters connected with the department of science in which he is engaged.

The subject which I have selected for the valedictory address

[1] Abstract of the Presidential address delivered before the American Association for the Advancement of Science, at Washington, August 1891, by George Lincoln Goodale, M.D., LL.D., Fisher Professor of Natural History, Harvard University, Cambridge, Mass., U.S.A.

deals with certain industrial, commercial, and economic questions: nevertheless it lies wholly within the domain of botany. I invite you to examine with me some of the possibilities of economic botany.

Of course, when treating a topic which is so largely speculative as this, it is difficult and unwise to draw a hard and fast line between possibilities and probabilities. Nowadays possibilities are so often realized rapidly that they become accomplished facts before we are aware.

In asking what are the possibilities that other plants than those we now use may be utilized we enter upon a many-sided inquiry. Speculation is rife as to the coming man. May we not ask what plants the coming man will use?

There is an enormous disproportion between the total number of species of plants known to botanical science and the number of those which are employed by man.

The species of flowering plants already described and named are about one hundred and seven thousand. Acquisitions from unexplored or imperfectly explored regions may increase the aggregate perhaps one-tenth, so that we are within very safe limits in taking the number of existing species to be somewhat above one hundred and ten thousand.

Now if we should make a comprehensive list of all the flowering plants which are cultivated on what we may call a fairly large scale at the present day, placing therein all food and forage plants, all those which are grown for timber and cabinet woods, for fibres and cordage, for tanning materials, dyes, resins, rubber, gums, oils, perfumes, and medicines, we could bring together barely three hundred species. If we should add to this short catalogue all the species, which, without cultivation, can be used by man, we should find it considerably lengthened. A great many products of the classes just referred to are derived in commerce from wild plants, but exactly how much their addition would extend the list, it is impossible in the present state of knowledge to determine. Every enumeration of this character is likely to contain errors from two sources: first, it would be sure to contain some species which have outlived their real usefulness; and, secondly, owing to the chaotic condition of the literature of the subject, omissions would occur.

But after all proper exclusions and additions have been made, the total number of species of flowering plants utilized to any considerable extent by man in his civilized state does not exceed, in fact it does not quite reach, one per cent.

The disproportion between the plants which are known and those which are used becomes much greater when we take into account the species of flowerless plants also. Of the five hundred ferns and their allies we employ for other than decorative purposes only five; the mosses and liverworts, roughly estimated at five hundred species, have only four which are directly used by man. There are comparatively few Algæ, Fungi, or Lichens which have extended use.

Therefore, when we take the flowering and flowerless together, the percentage of utilized plants falls far below the estimate made for the flowering alone.

Such a ratio between the number of species known and the number used justifies the inquiry which I have proposed for discussion at this time—namely, can the short list of useful plants be increased to advantage? If so, how?

This is a practical question; it is likewise a very old one. In one form or another, by one people or another, it has been asked from early times. In the dawn of civilization, mankind inherited from savage ancestors certain plants, which had been found amenable to simple cultivation and the products of these plants supplemented the spoils of the chase and of the sea. The question which we ask now was asked then. Wild plants were examined for new uses; primitive agriculture and horticulture extended their bounds in answer to this inquiry. Age after age has added slowly and cautiously to the list of cultivable and utilizable plants, but the aggregate additions have been, as we have seen, comparatively slight.

The question has thus no charm of novelty, but it is as practical to-day as in early ages. In fact, at the present time, in view of all the appliances at the command of modern science and under the strong light cast by recent biological and technological research, the inquiry which we propose assumes great importance. One phase of it is being attentively and systematically regarded in the great experiment stations, another phase is being studied in the laboratories of chemistry and pharmacy, while still another presents itself in the museums of economic botany.

Our question may be put in other words, which are even more practical. What present likelihood is there that our tables may, one of these days, have other vegetables, fruits, and cereals, than those which we use now? What chance is there that new fibres may supplement or even replace those which we spin and weave, that woven fabrics may take on new vegetable colours, that flowers and leaves may yield new perfumes and flavours? What probability is there that new remedial agents may be found among plants neglected or now wholly unknown? The answer which I shall attempt is not in the nature of a prophecy; it can claim no rank higher than that of a reasonable conjecture.

At the outset it must be said that synthetic chemistry has made and is making some exceedingly short cuts across this field of research, giving us artificial dyes, odours, flavours, and medicinal substances, of such excellence that it sometimes seems as if before long the old-fashioned chemical processes in the plant itself would play only a subordinate part. But although there is no telling where the triumphs of chemical synthesis will end, it is not probable that it will ever interfere essentially with certain classes of economic plants. It is impossible to conceive of a synthetic fibre or a synthetic fruit. Chemistry gives us fruit-ethers and fruit-acids, and after a while may provide us with a true artificial sugar and amorphous starch; but artificial fruits worth the eating or artificial fibres worth the spinning are not coming in our day.

Despite the extraordinary achievements of synthetic chemistry, the world must be content to accept, for a long time to come, the results of the intelligent labour of the cultivator of the soil and the explorer of the forest. Improvement of the good plants we now utilize, and the discovery of new ones, must remain the care of large numbers of diligent students and assiduous workmen. So that, in fact, our question resolves itself into this: Can these practical investigators hope to make any substantial advance?

It seems clear that, except in modern times, useful plants have been selected almost wholly by chance, and it may well be said that a selection by accident is no selection at all. Nowadays, the new selections are based on analogy. One of the most striking illustrations of the modern method is afforded by the utilization of bamboo fibre for electric lamps.

Some of the classes of useful plants must be passed by without present discussion; others alluded to slightly, while still other groups fairly representative of selection and improvement will be more fully described. In this latter class would naturally come, of course, the food-plants known as

I. THE CEREALS.

Let us look first at these.

The species of grasses which yield these seed-like fruits, or as we might call them for our purpose seeds, are numerous; twenty of them are cultivated largely in the Old World, but only six of them are likely to be very familiar to you—namely, wheat, rice, barley, oats, rye, and maize. The last of these is of American origin, despite doubts which have been cast upon it. It was not known in the Old World until after the discovery of the New. It has probably been very long in cultivation. The others all belong to the Old World. Wheat and barley have been cultivated from the earliest times; according to De Candolle, the chief authority in these matters, about four thousand years. Later came rye and oats, both of which have been known in cultivation for at least two thousand years. Even the shorter of these periods gives time enough for wide variation, and as is to be expected there are numerous varieties of them all. For instance, Vilmorin, in 1880, figured sixty-six varieties of wheat with plainly distinguishable characters.

If the Chinese records are to be trusted, rice has been cultivated for a period much longer than that assigned by our history and traditions to the other cereals, and the varieties are correspondingly numerous. It is said that in Japan above three hundred varieties are grown on irrigated lands, and more than one hundred on uplands.

With the possible exception of rice, not one of the species of cereals is certainly known in the wild state.

It is out of our power to predict how much time would elapse before satisfactory substitutes for our cereals could be found. In the improvement of the grains of grasses other than those which have been very long under cultivation, experiments have been few, scattered, and indecisive. Therefore we are as badly off for time-ratios as are the geologists and archæologists in their statements of elapsed periods. It is impossible for us to ignore the fact that there appear to be occasions in the life of

a species when it seems to be peculiarly susceptible to the influences of surroundings. A species, like a carefully laden ship, represents a balancing of forces within and without. Disturbance may come through variation from within, as from a shifting of the cargo, or in some cases from without. We may suppose both forces to be active in producing variation, a change in the internal condition rendering the plant more susceptible to any change in its surroundings. Under the influence of any marked disturbance, a state of unstable equilibrium may be brought about, at which times the species as such is easily acted upon by very slight agencies.

One of the most marked of these derangements is a consequent of cross-breeding within the extreme limits of varieties. The resultant forms in such cases can persist only by close breeding or by propagation from buds or the equivalents of buds. Disturbances like these arise unexpectedly in the ordinary course of nature, giving us sports of various kinds. These critical periods, however, are not unwelcome, since skilful cultivators can take advantage of them. In this very field much has been accomplished. An attentive study of the sagacious work done by Thomas Andrew Knight shows to what extent this can be done. But we must confess that it would be absolutely impossible to predict with certainty how long or how short would be the time before new cereals or acceptable equivalents for them would be provided. Upheld by the confidence which I have in the intelligence, ingenuity, and energy of our experiment stations, I may say that the time would not probably exceed that of two generations of our race, or half a century.

In now laying aside our hypothetical illustration, I venture to ask why it is that our experiment stations, and other institutions dealing with plants and their improvement, do not undertake investigations like those which I have sketched? Why are not some of the grasses other than our present cereals studied with reference to their adoption as food grains? One of these species will naturally suggest itself to you all—namely, the wild rice of the lakes. Observations have shown that, were it not for the difficulty of harvesting these grains, which fall too easily when they are ripe, they might be utilized. But attentive search might find or educe some variety of *Zizania*, with a more persistent grain and a better yield. There are two of our seashore grasses which have excellent grains, but are of small yield. Why are not these, or better ones which might be suggested by observation, taken in hand?

The reason is plain. We are all content to move along in lines of least resistance, and are disinclined to make a fresh start. It is merely leaving well enough alone, and so far as the cereals are concerned it is indeed well enough. The generous grains of modern varieties of wheat and barley compared with the well-preserved charred vestiges found in Greece by Schliemann, and in the lake-dwellings, are satisfactory in every respect. Improvements, however, are making in many directions; and in the cereals we now have, we possess far better and more satisfactory material for further improvement both in quality and as regards range of distribution than we could reasonably hope to have from other grasses.

From the cereals we may turn to the interesting groups of plants comprised under the general term

II. Vegetables.

Under this term it will be convenient for us to include all plants which are employed for culinary purposes, or for table use, such as salads and relishes.

The potato and sweet potato, the pumpkin and squash, the red or capsicum peppers, and the tomato, are of American origin.

All the others are, most probably, natives of the Old World. Only one plant coming in this class has been derived from Southern Australasia—namely, New Zealand spinach (*Tetragonia*).

Among the vegetables and salad-plants longest in cultivation we may enumerate the following: turnip, onion, cabbage, purslane, the large bean (Faba), chick-pea, lentil, and one species of pea (garden-pea). To these an antiquity of at least 4000 years is ascribed.

Next to these, in point of age, come the radish, carrot, beet, garlic, garden-cress, and celery, lettuce, asparagus, and the leek. Three or four leguminous seeds are to be placed in the same category, as are also the black peppers.

Of more recent introduction the most prominent are: the parsnip, oyster-plant, parsley, artichoke, endive, and spinach.

From these lists I have purposely omitted a few which belong exclusively to the tropics, such as certain yams.

The number of varieties of these vegetables is astounding. It is, of course, impossible to discriminate between closely allied varieties which have been introduced by gardeners and seedsmen under different names, but which are essentially identical, and we must therefore have recourse to a conservative authority, Vilmorin, from whose work a few examples have been selected. The varieties which he accepts are sufficiently well distinguished to admit of description, and in most instances of delineation, without any danger of confusion. The potato has, he says, innumerable varieties, of which he accepts forty as easily distinguishable and worthy of a place in a general list; but he adds also a list, comprising, of course, synonyms, of thirty-two French, twenty-six English, nineteen American, and eighteen German varieties. The following numbers speak for themselves, all being selected in the same careful manner as those of the potato: celery more than twenty; carrot more than thirty; beet, radish, and potato, more than forty; lettuce and onion more than fifty; turnip more than seventy; cabbage, kidney-bean, and garden pea, more than one hundred.

The amount of horticultural work which these numbers represent is enormous. Each variety established as a race (that is, a variety which comes true to seed) has been evolved by the same sort of patient care and waiting which we have seen is necessary in the case of cereals, but the time of waiting has not been as a general thing so long.

In the case of the cabbage there are important morphological changes like those to which Prof. Bailey has called attention in the case of the tomato. Suppose we are strolling along the beach at some of the seaside resorts of France, and should fall in with this coarse cruciferous plant, with its sprawling leaves and strong odour. Would there be anything in its appearance to lead us to search for its hidden merit as a food-plant? What could we see in it which would give it a preference over a score of other plants at our feet? Again, suppose we are journeying in the high lands of Peru, and should meet with a strong-smelling plant of the nightshade family, bearing a small irregular fruit, of sub-acid taste and of peculiar flavour. We will further imagine that the peculiar taste strikes our fancy, and we conceive that the plant has possibilities as a source of food. We should be led by our knowledge of the potato, probably a native of the same region, to think that this allied plant might be safely transferred to a northern climate, but would there be promise of enough future usefulness, in such a case as this, to warrant our carrying the plant north as an article of food? Suppose, further, we should ascertain that the fruit in question was relished not only by the natives of its home, but that it had found favour among the tribes of South Mexico and Central America, and had been cultivated by them until it had attained a large size; should we be strengthened in our venture? Let us go one step further still. Suppose that, having decided upon the introduction of the plant, and having urged everybody to try it, we should find it discarded as a fruit, but taking a place in gardens as a curiosity under an absurd name, or as a basis for preserves and pickles; should we not look upon our experiment in the introduction of this new plant as a failure? This is not a hypothetical case.

The tomato, the plant in question, was cultivated in Europe as long ago as 1554; it was known in Virginia in 1781 and in the Northern States in 1785; but it found its way into favour slowly, even in this land of its origin. A credible witness states that in Salem it was almost impossible to induce people to eat or even taste of the fruit. And yet, as you are well aware, its present cultivation on an enormous scale in Europe and this country is scarcely sufficient to meet the increasing demand.

Before asking specifically in what direction we shall look for new vegetables, I must be pardoned for calling attention, in passing, to a very few of the many which are already in limited use in Europe and this country, but which merit a wider employment. Cardon, or cardoon; celeriac, or turnip-rooted celery; fetticus, or corn-salad; martynia; salsify; sea-kale; and numerous small salads, are examples of neglected treasures of the vegetable garden.

The following, which are even less known, may be mentioned as fairly promising;—

(1) *Arracacia esculenta*, called Arracacha, belonging to the parsley family. It is extensively cultivated in some of the northern States of South America. The stems are swollen near

the base, and produce tuberous enlargements filled with an excellent starch. Although the plant is of comparatively easy cultivation, efforts to introduce it into Europe have not been successful, but it is said to have found favour in both the Indies, and may prove useful in our Southern States.

(2) *Ullucus* or Ollucus, another tuberous-rooted plant from nearly the same region, but belonging to the beet or spinach family. It has produced tubers of good size in England, but they are too waxy in consistence to dispute the place of the better tubers of the potato. The plant is worth investigating for our hot dry lands.

(3) A tuber-bearing relative of our common hedge-nettle, or *Stachys*, is now cultivated on a large scale at Crosnes, in France, for the Paris market. Its name in Paris is taken from the locality where it is now grown for use. Although its native country is Japan, it is called by some seedsmen Chinese artichoke. At the present stage of cultivation, the tubers are small and are rather hard to keep, but it is thought "that both of these defects can be overcome or evaded." Experiments indicate that we have in this species a valuable addition to our vegetables.

We must next look at certain other neglected possibilities.

Dr. Edward Palmer, whose energy as a collector and acuteness as an observer are known to you all, has brought together very interesting facts relative to the food-plants of our North American aborigines. Among the plants described by him there are a few which merit careful investigation. Against all of them, however, there lie the objections mentioned before, namely:—

(1) The long time required for their improvement, and

(2) The difficulty of making them acceptable to the community, involving

(3) The risk of total and mortifying failure.

In 1854 the late Prof. Gray called attention to the remarkable relations which exist between the plants of Japan and those of our eastern coast. You will remember that he not only proved that the plants of the two regions had a common origin, but also emphasized the fact that many species of the two countries are almost identical. It is to that country which has yielded us so many useful and beautiful plants that we turn for new vegetables to supplement our present food resources. One of these plants—namely, *Stachys*—has already been mentioned as promising. There are others which are worth examination and perhaps acquisition.

One of the most convenient places for a preliminary examination of the vegetables of Japan is at the railroad stations on the longer lines—for instance, that running from Tokio to Kobe. For native consumption there are prepared luncheon boxes of two or three stories, provided with the simple and yet embarrassing chopsticks. It is worth the shock it causes one's nerves to invest in these boxes and try the vegetable contents. The bits of fish, flesh, and fowl which one finds therein can be easily separated and discarded, upon which there will remain a few delicacies. The pervading odour of the box is that of aromatic vinegar. The generous portion of boiled rice is of excellent quality, with every grain well softened and distinct, and this without anything else would suffice for a tolerable meal. In the boxes which have fallen under my observation there were sundry boiled roots, shoots, and seeds which were not recognizable by me in their cooked form. Prof. Georgeson, formerly of Japan, has kindly identified some of these for me, but he says, "There are doubtless many others used occasionally."

One may find sliced lotus roots, roots of large burdock, lily bulbs, shoots of ginger, pickled green plums, beans of many sorts, boiled chestnuts, nuts of the gingko tree, pickled greens of various kinds, dried cucumbers, and several kinds of seaweeds. Some of the leaves and roots are cooked in much the same manner as beet-roots and beet-leaves are by us, and the general effect is not unappetizing. The boiled shoots are suggestive of only the tougher ends of asparagus. On the whole, I do not look back on Japanese railway luncheons with any longing which would compel me to advocate the indiscriminate introduction of the constituent vegetables here.

But when the same vegetables are served in native inns, under more favourable culinary conditions, without the flavour of vinegar and of the pine wood of the luncheon boxes, they appear to be worthy of a trial in our horticulture, and I therefore deal with one or two in greater detail.

Prof. Georgeson, whose advantages for acquiring a knowledge of the useful plants of Japan have been unusually good, has placed me under great obligations by communicating certain facts regarding some of the more promising plants of Japan which are not now used here. It should be said that several of these plants have already attracted the notice of the Agricultural Department in this country.

The soy bean (*Glycine hispida*). This species is known here to some extent, but we do not have the early and best varieties. These beans replace meat in the diet of the common people.

Mucuna (*Mucuna capitata*) and dolichos (*Dolichos cultratus*) are pole beans possessing merit.

Dioscorea. There are several varieties with palatable roots. Years ago one of these was spoken of by the late Dr. Gray as possessing "excellent roots, if one could only dig them."

Colocasia antiquorum has tuberous roots, which are nutritious.

Conophallus Konjak has a large bulbous root, which is sliced, dried, and beaten to a powder. It is an ingredient in cakes.

Aralia cordata is cultivated for the shoots, and used as we use asparagus.

Œnanthe stolonifera and *Cryptotænia canadensis* are palatable salad plants, the former being used also as greens.

III. FRUITS.

Botanically speaking, the cereal grains of which we have spoken are true fruits—that is to say, are ripened ovaries, but for all practical purposes they may be regarded as seeds. The fruits of which mention is now to be made are those commonly spoken of in our markets as fruits.

First of all, attention must be called to the extraordinary changes in the commercial relations of fruits by two direct causes—

(1) The canning industry, and

(2) Swift transportation by steamers and railroads.

The effects of these two agencies are too well known to require more than this passing mention. By them the fruits of the best fruit-growing countries are carried to distant lands in quantities which surprise all who see the statistics for the first time. The ratio of increase is very startling. Take, for instance, the figures given by Mr. D. Morris, at the time of the great Colonial and Indian Exhibition in London. Compare double decades of years—

	£
1845	886,888
1865	3,185,984
1885	7,587,523

In the Colonial Exhibition at London, in 1886, fruits from the remote colonies were exhibited under conditions which proved that, before long, it may be possible to place such delicacies as the cherimoyer, the sweet-cup, sweet-sop, rambutan, mango, and mangosteen, at even our most northern seaports. Furthermore, it seems to me likely that, with an increase in our knowledge with regard to the microbes which produce decay, we may be able to protect the delicate fruits from injury for any reasonable period. Methods which will supplement refrigeration are sure to come in the very near future, so that even in a country so vast as our own, the most perishable fruits will be transported through its length and breadth without harm.

The canning industry and swift transportation are likely to diminish zeal in searching for new fruits, since, as we have seen in the case of the cereals, we are prone to move in lines of least resistance, and leave well enough alone.

To what extent are our present fruits likely to be improved? Even those who have watched the improvement in the quality of some of our fruits, like oranges, can hardly realize how great has been the improvement within historic times in the character of certain pears, apples, and so on.

The term historic is used advisedly, for there are pre-historic fruits which might serve as a point of departure in the consideration of the question. In the ruins of the lake-dwellings in Switzerland, charred apples have been found, which are in some cases plainly of small size, hardly equalling ordinary crab apples. But, as Dr. Sturtevant has shown, in certain directions there has been no marked change of type—the change is in quality.

In comparing the earlier descriptions of fruits with modern accounts, it is well to remember that the high standards by which fruits are now judged are of recent establishment. Fruits which would once have been esteemed excellent would to-day be passed by as unworthy of regard.

It seems probable that the list of seedless fruits will be materially lengthened, provided our experimental horticulturists make use of the material at their command. The common fruits which have very few or no seeds are the banana, pineapple, and certain oranges. Others mentioned by Mr. Darwin as well known are the bread-fruit, pomegranate, arazole or Neapolitan medlar, and date-palms. In commenting upon these fruits, Mr. Darwin says that most horticulturists "look at the great size and anomalous development of the fruit as the cause, and sterility as the result," but he holds the opposite view as more probable—that is, that the sterility, coming about gradually, leaves free for other growth the abundant supply of building material which the forming seed would otherwise have. He admits, however, that "there is an antagonism between the two forms of reproduction, by seeds and by buds, when either is carried to an extreme degree, which is independent of any incipient sterility."

Most plant-hybrids are relatively infertile, but by no means wholly sterile. With this sterility there is generally augmented vegetative vigour, as shown by Nägeli. Partial or complete sterility, and corresponding luxuriance of root, stem, leaves, and flower may come about in other obscure ways, and such cases are familiar to botanists. Now, it seems highly probable that, either by hybridizing directed to this special end, or by careful selection of forms indicating this tendency to the correlated changes, we may succeed in obtaining important additions to our seedless or nearly seedless plants. Whether the ultimate profit would be large enough to pay for the time and labour involved is a question which we need not enter into; there appears to me no reasonable doubt that such efforts would be successful. There is no reason in the nature of things why we should not have strawberries without the so-called seeds; blackberries and raspberries, with only delicious pulp; and large grapes as free from seeds as the small ones which we call "currants," but which are really grapes from Corinth.

These, and the coreless apples and pears of the future, the stoneless cherries and plums, like the common fruits beforementioned, must be propagated by bud-division, and be open to the tendency to diminished strength said to be the consequence of continued bud-propagation. But this bridge need not be crossed until we come to it. Bananas have been perpetuated in this way for many centuries, and pineapples since the discovery of America, so that the borrowed trouble alluded to is not threatening.

It is absolutely necessary to recollect that, in most cases, variations are slight. Dr. Masters and Mr. Darwin have called attention to this, and have adduced many illustrations, all of which show the necessity of extreme patience and caution. The general student curious in such matters can have hardly any task more instructive than the detection of the variations in such common plants as the blueberry, the wild cherry, or the like. It is an excellent preparation for a practical study of the variations in our wild fruits suitable for selection.

It was held by the late Dr. Gray that the variations in nature by which species have been evolved were led along useful lines—a view which Mr. Darwin regretted he could not entertain. However this may be, all acknowledge that, by the hand of the cultivator, variations can be led along useful lines; and, furthermore, the hand which selects must uphold them in their unequal strife. In other words, it is one thing to select a variety, and another to assist it in maintaining its hold upon existence. Without the constant help of the cultivator who selects the useful variety, there comes a reversion to the ordinary specific type which is fitted to cope with its surroundings.

I think you can agree with me that the prospect for new fruits and for improvements in our established favourites is fairly good.

IV. TIMBERS AND CABINET WOODS.

Can we look for new timbers and cabinet woods? Comparatively few of those in common use are of recent introduction. Attempts have been made to bring into great prominence some of the excellent trees of India and Australia which furnish wood of much beauty and timber of the best quality. A large proportion of all the timbers of the South Seas are characterized by remarkable firmness of texture and high specific gravity. The same is noticed in many of the woods of the Indies. A few of the heavier and denser sorts, like Jarrah, of West Australia, and Sabicu of the Caribbean Islands, have met with deserved favour in England, but the cost of transportation militates against them. It is a fair question whether, in certain parts of our country, these trees, and others which can be utilized for veneers, may not be cultivated to advantage. Attention should be again called to the fact that many plants succeed far better in localities which are remote from their origin, but where they find conditions substantially like those which they have left. This fact, to which we must again refer in detail with regard to certain other classes of plants, may have some bearing upon the introduction of new timber trees. Certain drawbacks exist with regard to the timber of some of the more rapidly growing hard-wood trees which have prevented their taking a high place in the scale of values in mechanical engineering.

One of the most useful soft-wooded trees in the world is the Kauri. It is restricted in its range to a comparatively small area in the North Island of New Zealand. It is now being cut down with a recklessness which is as prodigal and shameful as that which has marked our own treatment of forests here. It should be said, however, that this destruction is under protest; in spite of which it would seem to be a question of only a few years when the great Kauri groves of New Zealand will be a thing of the past. Our energetic Forest Department has on its hands problems just like this which perplexes one of the new lands of the South. The task in both cases is double: to preserve the old treasures and to bring in new.

There is no department of economic botany more promising in immediate results than that of arboriculture.

V. VEGETABLE FIBRES.

The vegetable fibres known to commerce are either plant hairs, of which we take cotton as the type, or filaments of bast-tissue, represented by flax. No new plant hairs have been suggested which can compete in any way for spinning with those yielded by the species of Gossypium, or cotton, but experiments more or less systematic and thorough are being carried on with regard to the improvement of the varieties of the species. Plant hairs for the stuffing of cushions and pillows need not be referred to in connection with this subject.

Countless sorts of plants have been suggested as sources of good bast-fibres for spinning and for cordage, and many of these make capital substitutes for those already in the factories. But the questions of cheapness of production, and of subsequent preparation for use, have thus far militated against success. There may be much difference between the profits promised by a laboratory experiment and those resulting from the same process conducted on a commercial scale. The existence of such differences has been the rock on which many enterprises seeking to introduce new fibres have been wrecked.

In dismissing this portion of our subject it may be said that a process for separating fine fibres from undesirable structural elements and from resin-like substances which accompany them is a great desideratum. If this were supplied, many new species would assume great prominence at once.

VI. TANNING MATERIALS.

What new tanning materials can be confidently sought for? In his "Useful Native Plants of Australia," Mr. Maiden describes over thirty species of "wattles" or acacias, and about half as many eucalypts, which have been examined for the amount of tanning material contained in the bark. In all, eighty-seven Australian species have been under examination. Besides this, much has been done looking in the same direction at the suggestion and under the direction of Baron von Mueller, of Victoria. This serves to indicate how great is the interest in this subject, and how wide is the field in our own country for the introduction of new tanning plants.

It seems highly probable, however, that artificial tanning substances will at no distant day replace the crude matters now employed.

VII. RESINS, &c.

Resins, oils, gums, and medicines from the vegetable kingdom would next engage our attention if they did not seem rather too technical for this occasion, and to possess an interest on the whole somewhat too limited. But an allied substance may serve to represent this class of products and indicate the drift of present research.

India Rubber.—Under this term are included numerous sub-

stances which possess a physical and chemical resemblance to each other. An Indian Ficus, the early source of supply, soon became inadequate to furnish the quantity used in the arts even when the manipulation of rubber was almost unknown. Later supplies came from *Hevea* of Brazil, generally known as Para rubber, and from *Castilloa*, sometimes called Central American rubber, and from *Manihot Glaziovii*, Ceara rubber. Not only are these plants now successfully cultivated in experimental gardens in the tropics, but many other rubber-yielding species have been added to the list. The Landolphias are among the most promising of the whole: these are the African rubbers. Now in addition to these, which are the chief source of supply, we have *Willughbeia*, from the Malayan Peninsula, *Leuconotis*, *Chilocarpus*, *Alstonia*, *Forsteronia*, and a species of a genus formerly known as *Urostigma*, but now united with *Ficus*. These names, which have little significance as they are here pronounced in passing, are given now merely to impress upon our minds the fact that the sources of a single commercial article may be exceedingly diverse. Under these circumstances search is being made not only for the best varieties of these species but for new species as well.

There are few excursions in the tropics which possess greater interest to a botanist who cares for the industrial aspects of plants than the walks through the Gardens at Buitenzorg in Java and at Singapore. At both these stations the experimental gardens lie at some distance from the great Gardens which the tourist is expected to visit, but the exertion well repays him for all discomfort. Under the almost vertical rays of the sun, are here gathered the rubber-yielding plants from different countries, all growing under conditions favourable for decisions as to their relative value. At Buitenzorg a well-equipped laboratory stands ready to answer practical questions as to quality and composition of their products, and year by year the search extends.

I mention this, not as an isolated example of what is being accomplished in commercial botany, but as a fair illustration of the thoroughness with which the problems are being attacked. It should be further stated that at the Garden in question assiduous students of the subject are eagerly welcomed, and are provided with all needed appliances for carrying on technical, chemical, and pharmaceutical investigations. Therefore I am justified in saying that there is every reason for believing that in the very near future new sources of our most important products will be opened up, and new areas placed under successful cultivation.

At this point, attention must be called to a very modest and convenient hand-book on the "Commercial Botany of the Nineteenth Century," by Mr. Jackson, of the Botanical Museum attached to the Royal Gardens, Kew, which not only embodies a great amount of well-arranged information relative to the new useful plants, but is, at the same time, a record of the existing state of things in all these departments of activity.

VIII. Fragrant Plants.

Another illustration of our subject might be drawn from a class of plants which repays close study from a biological point of view—namely, those which yield perfumes.

In speaking of the future of our fragrant plants we must distinguish between those of commercial value and those of purely horticultural interest. The former will be less and less cultivated in proportion as synthetic chemistry by its manufacture of perfumes replaces the natural by the artificial products; for example, coumarin, vanillin, nerolin, heliotropin, and even oil of wintergreen.

When, however, one has seen that the aromatic plants of Australia are almost free from attacks of insects and fungi, and has learned to look on the impregnating substances in some cases as protective against predatory insects and small foes of all kinds, and in others as fungicidal, he is tempted to ask whether all the substances of marked odour which we find in certain groups of plants may not play a similar *rôle*.

It is a fact of great interest to the surgeon that in many plants there is associated with the fragrant principle a marked antiseptic or fungicidal quality; conspicuous examples of this are afforded by species of *Eucalyptus*, yielding eucalyptol, *Styrax*, yielding styrone, *Thymus*, yielding thymol. It is interesting to note, too, that some of these most modern antiseptics were important constituents in the balsamic vulneraries of the earliest surgery.

Florists' plants and the floral fashions of the future constitute an engaging subject, which we can touch only lightly. It is reasonably clear that while the old favourite species will hold their ground in the guise of improved varieties, the new introductions will come in the shape of plants with flowering branches which retain their blossoms for a somewhat long period, and especially those in which the flowers precede the leaves. In short, the next real fashion in our gardens is probably to be the flowering shrub and flowering tree, like those which are such favourites in the country from which the Western world has gladly taken the gift of the chrysanthemum.

Twice each year, of late, a reception has been held by the Emperor and Empress of Japan. The receptions are in autumn and in the spring. That in the autumn, popularly known as the Emperor's reception, has for its floral decorations the myriad forms of the national flower, the chrysanthemum; that which is given in spring, the Empress's reception, comes when the cherry blossoms are at their best. One has little idea of the wealth of beauty in masses of flowering shrubs and trees, until he has seen the floral displays in the Imperial Gardens and the Temple grounds in Tokio.

Conclusion.

Lack of time renders it impossible to deal with the questions which attach themselves to our main question, especially as to the limits of effect which cultivation may produce. We cannot touch the problem of inheritance of acquired peculiarities, or the manner in which cultivation predisposes the plant to innumerable modifications. Two of these modifications may be mentioned in passing, because they serve to exemplify the practical character of our subject.

Cultivation brings about in plants very curious morphological changes. For example, in the case of a well-known vegetable the number of metamorphosed type-leaves forming the ovary is two, and yet under cultivation the number increases irregularly until the full number of units in the type of the flower is reached. Prof. Bailey, of Cornell, has called attention to some further interesting changes in the tomato, but the one mentioned suffices to illustrate the direction of variation which plants under cultivation are apt to take. Monstrosities are very apt to occur in cultivated plants, and under certain conditions may be perpetuated in succeeding generations, thus widening the field from which utilizable plants may be taken.

Another case of change produced by cultivation is likewise as yet wholly unexplained, although much studied—namely, the mutual interaction of scion and stock in grafting, budding, and the like. It is probable that a further investigation of this subject may yet throw light on new possibilities in plants.

We have now arrived at the most practical question of all, namely—

In what way can the range of commercial botany be extended? In what manner, or by what means, can the introduction of new species be hastened?

It is possible that some of you are aware of the great amount of uncoordinated work which has been done and is now in hand in the direction of bringing in new plants.

The competition between the importers of new plants is so great both in the Old World and the New that a very large proportion of the species which would naturally commend themselves for the use of florists, for the adornment of greenhouses, or for commerical ends, have been at one time or another brought before the public or are being accumulated in stock. The same is true, although to a less extent, with regard to useful vegetables and fruit. Hardly one of those which we can suggest as desirable for trial has not already been investigated in Europe or this country, and reported on. The pages of our chemical, pharmaceutical, medical, horticultural, agricultural and trade journals, especially those of high grade, contain a wealth of material of this character.

But what is needed is this: that the promising plants should be systematically investigated under exhaustive conditions. It is not enough that an enthusiast here, or an amateur there, should give a plant a trial under imperfectly understood conditions, and then report success or failure. The work should be thorough, and every question answered categorically, so that we might be placed in possession of all the facts relative to the object experimented upon. But such an undertaking requires the co-operation of many different agencies. I shall venture to mention some of these.

In the first place, Botanic Gardens amply endowed for research. The Arnold Arboretum, the Shaw Garden, and the Washington Experimental Garden, are American illustrations of what is needed for this purpose. University gardens have their place in instruction, but cannot wisely undertake this kind of work.

In the second place, Museums and Laboratories of Economic Botany. Much good work in this direction has been done in this country by the National Museum and by the department in charge of the investigation of new plants. We need institutions like those at Kew in England, and at Buitenzorg in Java, which keep in close touch with all the world. The founding of an establishment on a scale of magnitude commensurate with the greatness and needs of our country is an undertaking which waits for some one of our wealthy men.

In the third place, Experiment Stations. These may, within the proper limits of their sphere of action, extend the study of plants beyond the established varieties to the species, and beyond the species to equivalent species in other genera. It is a matter of regret that so much of the energy displayed in these stations in this country, and we may say abroad, has not been more economically directed.

Great economy of energy must result from the recent change by which co-ordination of action is assured. The influence which the stations must exert on the welfare of our country and the development of its resources is incalculable.

In the last place, but by no means least, the co-operation of all who are interested in scientific matters, through their observation of isolated and associated phenomena connected with plants of supposed utility, and by the cultivation of such plants by private individuals, unconnected with any State, Governmental, or academic institutions.

By these agencies, wisely directed and energetically employed, the domains of commercial and industrial botany will be enlarged. To some of the possible results in these domains, I have endeavoured to call your attention.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

PROF. BONNEY will begin a course of about sixty lectures on geology at University College, London, on Tuesday, October 6, at noon, and a course of about eighteen lectures on geology for engineering students, on Monday, October 12, at 2 p.m. A class for students preparing for the B.Sc. degree in the University of London will meet on October 6 at 2 p.m.

THE prizes to the students at the medical school of St. Thomas's Hospital will be distributed to-day by Sir G. M. Humphry, F.R.S.

LECTURES will be delivered in Gresham College, Basinghall Street, E.C., on October 6, 7, 8, and 9, by Dr. E. Symes Thompson, Gresham Professor of Medicine, on influenza and its results.

SEVERAL series of lectures for which the Salop County Council has made arrangements have been begun. They are on chemistry, botany, geology, agricultural chemistry, management of stock, insect pests and crop diseases, mechanics, and principles of agriculture, and are being given in various parts of the county. Most of them are being delivered in connection with the Oxford University Extension Scheme.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 21.—M. Duchartre in the chair.—Admiral Mouchez made some remarks on the second volume of the Paris Observatory Star Catalogue, presented to the Academy. The Catalogue contains stars between the right ascensions 6h. and 12h., and about 500,000 observations made at Paris during the last fifty years have been utilized in its construction.—On the colour sensations excited in one eye by coloured light which illuminates the retina of the other, by M. A. Chauveau. From the experiments described it appears that the excitation of one retina by coloured light influences, not only the optic nerves of this retina, but also those of the opposite side, so that the latter are able to awaken the sensation of the colour employed whilst the excited retina only sees the complementary colour. Thus, if a white surface be observed for a short time through a bit of coloured glass, using only one eye, and screening the other, when the glass is taken away the white ground appears to be tinted with a colour complementary to that of the glass. This is an old experiment, but the point is that if the first eye be closed and the screened eye opened the white surface appears to be tinted with the same colour as the glass.—Observations of the asteroid discovered by Charlois on August 28, made with the *coudé* equatorial of Algiers Observatory, by M. F. Sy. Observations for position were made on August 31 and September 7.—Observations of Wolf's comet (1884 *e* III.) made with the *coudé* equatorial (0·36m. aperture) of Lyons Observatory, by M. G. Le Cadet. Observations for position were made on September 9, 10, 11, and 12.—On the partial eclipse of Jupiter's first satellite by the shadow of the second, by M. J. J. Landerer. This phenomenon occurred on August 14.—The metamorphoses of *Acridium peregrinum*, Oliv., by M. Charles Brongniart. The author has specially observed that locusts undergo various colour changes at different stages of their existence.—On the grafting of underground portions of plants, by M. Lucien Daniel.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Mechanics for Beginners; Part 1, Dynamics and Statics: Rev. J. B. Lock (Macmillan).—Manual of the Science of Religion: Prof. P. D. C. de la Saussaye; translated by B. S. Colyer-Fergusson (Longmans).—Solutions: Prof. Ostwald; translated by M. M. P. Muir (Longmans).—Principles and Practice of Plumbing: S. S. Hellyer (Bell).—Lunar Radiant Heat: O. Boeddicker (Williams and Norgate).—The Universal Atlas, Parts 1 to 6 (Cassell).—Mayhew's Illustrated Horse Doctor, revised and improved: J. I. Lupton (Griffith).—Foods for the Fat, 3rd edition: Dr. Yorke-Davies (Chatto).—On the Adjustment and Testing of Telescopic Objectives: T. Cooke and Sons (York, Johnson).—Die geographische Verbreitung der Säugetiere: Dr. A Nehring (Berlin, Pormetter).—De Klimaten der Voorwereld en de Geschiedenis der Zon: E. Dubois (Batavia, Ernst).—Economic Journal, No. 3 (Macmillan).—Journal of the Asiatic Society of Bengal, Vol. lix., Part 2, Nos. 4 and 5; Vol. lix., Part 2, Supplement No. 2; Vol. lx., Part 2, No. 1 (Calcutta).—Journal of Physiology, vol. xii., No. 4 (Cambridge).—Calendar of the University College of Wales, Aberystwyth, 1891–92 (Manchester, Cornish).—Psychology: F. S. Granger (Methuen).—Studies in Jewish Statistics: J. Jacobs (Nutt).—Diphtheria: Dr. R. Thorne Thorne (Macmillan).—Experiments in Aërodynamics: S. P. Langley (Washington).—The Story of the Heavens, 18th Edition: Sir R. S. Ball (Cassell).—Deutsche Seewarte—Indischer Ozean, Ein Atlas (Hamburg, Friederichsen).—Arithmetical Exercises in Chemistry: Dr. L. Dobbin (Edinburgh, Thin).—La Transcaucasie et la Péninsule D'Apchéron: C. S. Gulbenkian (Paris, Hachette).—Ueber die Finnländischen Rapakiwigesteine: J. J. Sederholm (Wien, Hölder).—Studien über Archäische Eruptivgesteine aus dem Südwestlichen Finnland: J. J. Sederholm (Wien, Hölder).—The Eocene and Oligocene Beds of the Paris Basin: Harris and Burrows (Stanford).—Versuch über die Erdgeschichtliche Entwickelung: Dr. G. Pfeffer (Hamburg, Friederichsen).

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